

THE  
PHILOSOPHICAL MAGAZINE:

COMPREHENDING  
THE VARIOUS BRANCHES OF SCIENCE,  
THE LIBERAL AND FINE ARTS,  
AGRICULTURE, MANUFACTURES,  
AND  
COMMERCE.

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BY ALEXANDER TILLOCH,

MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY, ETC. ETC.

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“Nec araneorum fane textus ideo melior, quia ex se fila gignunt. Nec noster vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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VOL. VIII.

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LONDON:

PRINTED BY DAVIS, TAYLOR, AND WILKS, CHANCERY-LANE,  
FOR ALEXANDER TILLOCH; and sold by Messrs. RICHARDSON,  
Cornhill; CADELL and DAVIES, Strand; DEBRETT, Piccadilly;  
MURRAY and HIGLEY, No. 32, Fleet-street; SYMONDS,  
Peter-noster Row; BELL, No. 148, Oxford-street;  
JERNOR and HOOD, Poultry; HARDING, No. 36,  
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Edinburgh; BRASH and REID, Glasgow;  
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# THE PHILOSOPHICAL MAGAZINE.

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OCTOBER 1800.

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I. *Thoughts on Colouring, and particularly with a Retrospect to the Method used by the Venetians in the Mechanical Part of the Art, and to their Method of Arranging the Tints.*

A theory founded on experiment, and not assumed, is always good for so much as it explains.

BURKE.

SIR,

SINCE I troubled you with a paper on the subject of Venetian colouring\*, I have paid particular attention to such pictures of that school as have occasionally come under my notice, and, in my inquiry, I have been much aided by those in the Orleans collection; but, from all the observations I have been able to make, I cannot help concluding that they never thought of the absorbent ground, on which I made some remarks in my last, and that the dark red ground never formed a part of the picture. Those conclusions I feel authorised in making from the general tendency in the absorbent grounds to produce a hardness, a thing never occurring in the pictures of the Venetians; and that the ground never entered into part of their pictures is evident from their being painted solid throughout, and *the shadows glazed on a body of colour*. I shall, with a view further to illustrate the subject, introduce (occasionally as quotations) such remarks on colouring as I made in my catalogue immediately on my viewing the pictures in the collection above alluded to.

\* See Vol. IV.

VOL. VIII.

B 2

On

On looking into the Venetian pictures (particularly those of Titian and Paul Veronese), I found that they had all been prepared with colder colours than the finishing ones, and that there was one general colour which prevailed throughout the shadows; this observation induced me to make the following remark: "the Venetians appear to have prepared their pictures in the flesh, and its shadow and draperies, in a colder state, and glazed or scumbled them with warmer colours."

One of the greatest considerations with the artist is to acquire the true tone of the shadow and its dusky shadow; to do this he must use such colours as will most readily sympathise with each other in mixing: to this end it will be found that ivory black, Indian red, and white, will form the best general shadow colour, which may in the flesh be improved with vermilion in the reflections. The ivory black, and white, improved with lake, produces a most beautiful pearly demi-tint for delicate flesh, and at the same time one of the truest and most clean that can be acquired. Ivory black with Indian red, or lake, glazes finely, and may be highly improved, when dry, with brown pink, or asphaltum, used with drying oil. This is very like the method I formerly mentioned\*, as practised by that great man Sir Joshua Reynolds, to acquire his shadows, and whose method of colouring was truly Venetian, accompanied with a *chiaro scuro* they were strangers to, and a taste in his portraits unexampled. Those who choose to try, will find their advantage in making the shadow colour of ivory black, vermilion, and white (as mentioned in Vol. IV.), which will work cleaner than any other combination; and which colours, with the addition of burnt ochre in dark complexions, and yellow ochre in the lights, will be quite sufficient for all the purposes in flesh for first and second colouring, or till the drawing is fixed; the whole being afterwards finished by glazing, to give depth to the shadows, and enrich and improve the colouring. Tintoret appears to have forwarded his pictures to nearly their full effect, with a pearly colour of black, red, and white, and to have acquired all his tints by glazing with a pure transparent colour, invariably leaving the lights white

\* See Vol. IV.



to give them brilliancy: Titian, on the contrary, *dead-coloured as near nature as possible*, only enriching his colours by glazing, and keeping the shadows as near of a tone as possible. The absurdity of using dark grounds must be obvious to every one who has had the least practice, from the tendency in the white, and all the lighter tints, to sink into the ground; which accounts for the darkness of many of the old pictures, and more especially those that have been painted thin. However desirable it may be to have one's pictures well coloured, its presence will by no means compensate for the loss of dignity of composition, accuracy of drawing or breadths: he that is content to please by the illusion of colour, or that flatters the eye by an industrious display of tints, must not expect to rank high in the art.

“ Mind, mind alone, (bear witness Earth and Heav'n!)

“ The living fountains in itself contains

“ Of beautiful and sublime: here hand in hand

“ Sit paramount the Graces.”

AKENSIDE.

I have often been led to think that the exquisite clearness observable in some of the pictures of Rubens, arises at this time (in a great degree) from his having painted them on white, or, at any rate, light grounds; the advantage of which will be obvious to any one that chooses to try the following experiment: that is, to spread any light colour (as a flesh colour for instance) on a ground prepared half white and half dark; in the course of two or three days, that on the white ground will appear clear, while the latter will have sunk many degrees into the under dark colour. Another advantage attending a light ground is, that the picture is not so liable to be injured by cleaning, not to mention its wearing better: two of the finest pictures, by Titian, in the Orleans collection (Diana and Acteon, and Diana and Calisto), bear strong testimony to the truth of the above observation; the former being in fine preservation, owing to its having been painted with a great body of colour; while the latter, having only a thin coat, the dark red ground appears through the flesh, to its irreparable injury. This evil would in a great measure have been avoided, had the ground been light;

light; and it is evident that the ground was not intended to make the least part of the effect in the picture originally, as the thinness in the colour above alluded to, looks as if owing to accident, and the haste of a man who could sell all the pictures he painted.

I have already mentioned that the best general shadow colour is ivory black, &c. which, if the lights and middle tints are coloured as near nature as possible, may be enriched by glazing; or the glazing may be done on the middle tints, and the lights of one general hue laid on them, which appears at times to have been the practice of Titian. The following remark on the picture of Diana and Calisto, may serve as a further illustration:—"There appears to be yellow in all the lights, even the blue; I therefore infer, that they considered the light as giving a colour, or of changing, in a small degree, the local colour of the objects, and that their principle of colouring was thus: *the lights on the objects partook of the colour of the light, the middle tints gave the local colour, and the shadows warm, and pretty much of one hue.*" A picture painted on the above plan, will have a harmonious and pleasant effect.

I wish the artists of any eminence would communicate the result of their practice; much good would arise from it. Fine writing is not a necessary requisite; for, in matters intended to instruct, clearness will more than compensate for want of elegance: such a conduct would contribute to remove many of the practical difficulties the student labours under; an inconvenience I myself have greatly felt.

It is very rarely that you see a distinct or whole colour in the Venetian pictures; as their blues seldom go beyond a violet, and the reds, a crimson. "In all the pictures in the room (Orleans collection) by this master (Titian), no one decisive colour is to be seen, as they are all broken to correspond with each other: that is, the red are broken with yellow, yellows with red, and so on. He appears to have been particularly careful not to introduce blue or cold colours, and, when they do occur, it is only in small quantities."

I would by no means have it understood that I wish to tie the artist down to a particular manner of colouring; one



of our professional evils is looking too much to art for its rules; but I believe a great source of the pleasure derived from the sight of the Venetian pictures, is their comfortable warm masses: great caution however will be required to distinguish between a glowing and a glaring colour; as we are apt to admire what is showy, before we can distinguish what is beautiful. I have frequently had occasion to remark the satisfaction expressed by the spectators on viewing a warmly coloured picture, and the degree of comfort it has appeared to impart: this has frequently led me to reflect on the application of colour to medical purposes, as in the ague or fever, or how much the mind might be tranquilised by the room being of a particular colour; every one is sensible of the effect of that beautiful amber-coloured light that at times prevails on our summer's evenings, and which is so highly congenial to health and life\*.

In all cases the good sense of the artist must be used, for the colouring that suits one subject will not agree with another; most of the Venetian pictures that I have seen, have been of the pleasing or ornamental cast, which require more decoration of colour than the higher walks of history, but I have never been able to prevail on myself to believe the most sublime subject would be hurt by grouping the colours. It would be worth inquiring how far the painter might be benefited (as the poet certainly is) by considering colours as indicative of the passions, and applying them accordingly: the fiery reds convey the idea of anger, and in this sense are often used:

“ Should intermitted vengeance arm again

“ His red right hand to plague us?

MILTON.

\* The fact is it is used medically, as in the case of weak eyes, but not on so extensive a plan as it might be; for instance, if the walls of the chambers where insane people are confined were to be plastered to an uniform surface, and coloured of a still green, as near the tranquil colour of grass as possible, it is probable it would contribute to compose the spirits; instead of which the sight is distracted, and in all probability the mind set to work in decyphering the fissures and irregularities in their surrounding old walls, which may ultimately end in provoking that propensity towards drawing expressed by many unfortunate people.

Addison

Addison, in his *Cato*, heightens his figure by “red with uncommon wrath.” Gray and the soft green convey the idea of tranquillity; and the dark blue gray, and brown, of sorrow and melancholy:

“ ————— me, goddess, bring,

“ To arched walks of twilight groves,

“ And shadows brown, that Sylvan loves,

“ Of pine ———.

*Milton's Il Penseroso.*

Mellow tones ever associate with gentle, and sharp ones with violent, emotions.

In the practice of glazing, the less oil is used the better; if the part is rubbed out, and wiped dry with a silk rag (an old silk handkerchief is good, but a piece of wash leather does best), then the colour should be driven as much as possible and often repeated if necessary, except it should be a colour that requires drying oil: and, I believe, on most occasions, if the part is hard, drying oil and turpentine, in equal parts, will be found as good a vehicle as any. In what the artist terms *scumbling*, or using an opaque colour thin, it should be driven with a stiff pencil, and as little oil as possible.

As a conclusion to the present paper, I shall add the sum of my remarks on the Venetian pictures in the Orleans collection:—“From the pictures of Titian and Paul Veronese I judge they were particularly careful to group the warm colours; to avoid the use of blue and cold ones; *never to suffer them to interrupt the principal mass of light, which with them is always composed of warm colours*; that the cold colours were sparingly introduced for the purpose of supporting the blue in the sky or distance, as in No. 123, 240, 249\*, and never on the principal object. In many of their pictures there is a total exclusion of the cold colours, as in 169 and 181; in others, where the colours of the objects are all warm, the skies are broken either with warm purple clouds, or the blue of the element reduced to gray, as in 49, 211,

\* 123, Holy family in a landscape, Titian; 240, Diana and Actæon, Titian; and 249, Diana and Calisto, Titian; 169, an Allegory; and 181, a ditto, Paul Veronese.



220 \*; their shadows prepared with a gray or pearly colour, enriched with glazing: their draperies glazed; flesh glazed, and scumbled with rich virgin tints; and the whole harmonised by the high lights being touched on with a warm yellow colour, nearly of the tone of the high lights of the flesh, as in remark on No. 249 †." I cannot help adding, there were not any pictures in the Orleans collection which (in point of colour) looked so agreeable as the Venetian, owing to their principal mass of colour being invariably warm; wherever the contrary occurred, the effect on the eye was unpleasant; as also when the great mass was disturbed by the intervention of a cold colour.

I remain, Sir, your humble servant,  
E. DAYES.

Francis-street,  
April 11, 1800.

*The Editor of the Philosophical Magazine.*

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II. *Experiments on the Refrangibility of the invisible Rays of the Sun.* By WILLIAM HERSCHEL, LL.D. F.R.S. ‡

IN that section of my former paper which treats of radiant heat, it was hinted, though from imperfect experiments, that the range of its refrangibility is probably more extensive than that of the prismatic colours; but, having lately had some favourable sunshine, and obtained a sufficient confirmation of the same, it will be proper to add the following experiments to those which have been given:—

I provided a small stand, with four short legs, and covered it with white paper §. On this I drew five lines, parallel to one end of the stand, at half an inch distance from each other, but so that the first of the lines might only be  $\frac{1}{4}$  of an

\* 249, Venus rising from the sea, Titian; 211, the education of Cupid, Titian; 220, Europa, Titian.

† Diana and Calisto, Titian.

‡ From the *Transactions of the Royal Society for 1800.*

§ See Plate I. fig. 2.

inch from the edge. These lines I intersected at right angles with three others; the second and third whereof were, respectively, at  $2\frac{1}{2}$  and at 4 inches from the first.

The same thermometers that have before been marked No. 1, 2, and 3, mounted upon their small inclined planes, were then placed so as to have the centres of the shadow of their balls thrown on the intersection of these lines. Now, setting my little stand upon a table, I caused the prismatic spectrum to fall with its extreme colour upon the edge of the paper, so that none might advance beyond the first line. In this arrangement, all the spectrum, except the vanishing last quarter of an inch, which served as a direction, passed down by the edge of the stand, and could not interfere with the experiments. I had also now used the precaution of darkening the window in which the prism was placed, by fixing up a thick dark green curtain, to keep out as much light as convenient.

The thermometers being perfectly settled at the temperature of the room, I placed the stand so that part of the red colour, refracted by the prism, fell on the edge of the paper, before the thermometer No. 1, and about half way, or  $1\frac{1}{4}$  inch, towards the second: it consequently did not come before that, or the third thermometer, both which were to be my standards. During the experiment, I kept the last termination of visible red carefully to the first line, as a limit assigned to it, by gently moving the stand when required; and found the thermometers, which were all placed on the second line, affected as follows:

No. 1.	No. 2.	No. 3.	} Here the thermometer No. 1 rose $6\frac{1}{2}$ degrees in 10 minutes, when its centre was placed $\frac{1}{2}$ inch beyond visible light.
45	45	44	
49	45	44	
51	$44\frac{3}{4}$	44	
$50\frac{1}{4}$	$43\frac{3}{4}$	$43\frac{1}{2}$	

In order to have a confirmation of this fact, I cooled the thermometer No. 1, and placed No. 2 in the room of it: I also put No. 3 in the place of No. 2, and No. 1 in that of No. 3; and, having exposed them as before, arranged on the second line, I had the following result:

No.



No. 2.	No. 3.	No. 1.	} Here the thermometer No. 2 rose $2\frac{3}{4}$ degrees in 12 mi- nutes; and being, as has been noticed before, much more sensible than No. 1,
44	44	45	
47	44	45	
$46\frac{3}{4}$	44	45	
$46\frac{3}{4}$	44	45	

it came to the temperature of its situation a short time; but I left it exposed longer, on purpose to be perfectly assured of the result. Its showing but  $2\frac{3}{4}$  degrees advance, when No. 1 showed  $6\frac{1}{2}$ , has also been accounted for before.

It being now evident that there was a refraction of rays coming from the sun, which, though not fit for vision, were yet highly invested with a power of occasioning heat, I proceeded to examine its extent as follows:

The thermometers were arranged on the third line instead of the second; and the stand was, as before, immersed up to the first, in the coloured margin of the vanishing red rays. The result was thus:

No. 1.	No. 2.	No. 3.	} Here the thermometer No. 1, rose $5\frac{1}{4}$ degrees in 13 mi- nutes, at 1 inch behind the visible light of the red rays.
46	46	$45\frac{3}{4}$	
50	$46\frac{1}{2}$	46	
$51\frac{3}{4}$	$46\frac{3}{4}$	$46\frac{1}{4}$	
$52\frac{1}{4}$	47	$46\frac{3}{4}$	

I placed now the thermometers on the fourth line instead of the third; and, proceeding as before, I had the following result:

No. 1.	No. 2.	No. 3.	} Therefore, the thermometer No. 1, rose $3\frac{1}{8}$ degree in 10 minutes, at $1\frac{1}{2}$ inch beyond the visible light of the red rays.
$48\frac{1}{4}$	$48\frac{1}{4}$	$47\frac{3}{4}$	
$51\frac{1}{2}$	$48\frac{3}{8}$	$47\frac{7}{8}$	

I might now have gone to the fifth line; but so fine a day, with regard to clearness of sky and perfect calmness, was not to be expected often at this time of the year; I therefore hastened to make a trial of the other extreme of the prismatic spectrum. This was attended with some difficulty, as the illumination of the violet rays is so feeble, that a precise termination of it cannot be perceived. However, as well as could be judged, I placed the thermometers one inch beyond the reach of the violet rays, and found the result as follows:

No. 1.	No. 2.	No. 3.	} Here the several indications of the thermometers, two of which, No. 1 and 2, were used as variable, while the third was kept as the standard, were read
48	48	$47\frac{3}{4}$	
48	48	$47\frac{3}{4}$	
48	$47\frac{1}{2}$	47	
$48\frac{1}{2}$	$47\frac{1}{2}$	47	
48	48	$47\frac{3}{4}$	

off during a time that lasted 12 minutes; but they afford, as may be seen by inspection, no ground for ascribing any of their small changes to other causes than the accidental disturbance which will arise from the motion of the air in a room where some employment is carried on.

I exposed the thermometer now to the line of the very first perceptible violet light, but so that No. 1 and 2 might again be in the illumination, while No. 3 remained a standard. The result proved as follows:

No. 1.	No. 2.	No. 3.	} Here the thermometer No. 1 rose 1 degree in 15 minutes; and No. 2 rose $\frac{1}{2}$ degree in the same time.
48	48	$47\frac{3}{4}$	
$48\frac{1}{2}$	48	$47\frac{3}{4}$	
$48\frac{3}{4}$	$48\frac{1}{2}$	$47\frac{3}{4}$	
49	$48\frac{1}{2}$	$47\frac{3}{4}$	

From these last experiments, I was now sufficiently persuaded, that no rays which might fall beyond the violet, could have any perceptible power either of illuminating or of heating; and that both these powers continued together throughout the prismatic spectrum, and ended where the faintest violet vanishes.

A very material point remained still to be determined, which was, the situation of the maximum of the heating power.

As I knew already that it did not lie on the violet side of the red, I began at the full red colour, and exposed my thermometers, arranged on a line, so as to have the ball of No. 1 in the midst of its rays, while the other two remained at the side, unaffected by them.

No. 1.	No. 2.	No. 3.	} Here the thermometer No. 1 rose 7 degrees in 10 minutes, by an exposure to the full red coloured rays.
$48\frac{1}{2}$	$48\frac{1}{2}$	48	
$55\frac{1}{2}$	$48\frac{1}{2}$	48	
$55\frac{1}{2}$	$48\frac{1}{2}$	48	

I drew back the stand, till the centre of the ball of No. 1, was just at the vanishing of the red colour, so that half its ball was within, and half without, the visible rays of the sun.

No.



No. 1.	No. 2.	No. 3.	} Here the thermometer No. 1 rose 8 degrees in 10 mi- nutes.
$48\frac{1}{2}$	$48\frac{1}{2}$	48	
$55\frac{1}{2}$	$48\frac{1}{2}$	48	
57	49	$48\frac{1}{2}$	

By way of not losing time, in order to connect these last observations the better together, I did not bring back the thermometer No. 1, to the temperature of the room, being already well acquainted with its rate of flowing, compared to that of No. 2, but went on to the next experiment, by withdrawing the stand, till the whole ball of No. 1 was completely out of the sun's visible rays, yet so as to bring the termination of the line of the red colour as near the outside of the ball as could be without touching it.

No. 1.	No. 2.	No. 3.	} Here the thermometer No. 1, rose, in 10 minutes, an- other degree higher than in its former situation it could be brought up to;
57	49	$48\frac{1}{2}$	
$58\frac{1}{2}$	$49\frac{3}{4}$	49	
59	$50\frac{1}{4}$	$49\frac{3}{4}$	
59	50	$49\frac{1}{2}$	

and was now 9 degrees above the standard. The ball of this thermometer, as has been noticed, is exactly half an inch in diameter; and its centre, therefore, was  $\frac{1}{4}$  inch beyond the visible illumination, to which no part of it was exposed.

It would not have been proper to compare these last observations with those taken at an earlier period this morning, in order to obtain a true maximum, as the sun was now more powerful than it had been at that time; for which reason I caused the line of termination of visible light, now to fall again just  $\frac{1}{2}$  inch from the centre of the ball, and had the following result:

No. 1.	No. 2.	No. 3.	} And here the thermometer No. 1, rose, in 16 minutes, $8\frac{3}{4}$ degrees, when its cen- tre was $\frac{1}{2}$ inch out of the visible rays of the sun.
$50\frac{1}{2}$	$50\frac{1}{2}$	50	
$57\frac{3}{4}$	50	$49\frac{1}{2}$	
$58\frac{1}{2}$	50	$49\frac{1}{2}$	
$58\frac{3}{4}$	50	$49\frac{1}{2}$	

Now, as before we had a rising of nine degrees, and here  $8\frac{3}{4}$ , the difference is almost too trifling to suppose that this latter situation of the thermometer was much beyond the maximum of the heating power; while, at the same time, the experiment sufficiently indicates that the place inquired after, need not be looked for at a greater distance.

It will now be easy to draw the result of these observations into a very narrow compass.

The first four experiments prove that there are rays coming from the sun which are less refrangible than any of those that affect the sight. They are invested with a high power of heating bodies, but with none of illuminating objects; and this explains the reason why they have hitherto escaped unnoticed.

My present intention is, not to assign the angle of the least refrangibility belonging to these rays, for which purpose more accurate, repeated, and extended experiments are required. But, at the distance of 52 inches from the prism, there was still a considerable heating power exerted by our invisible rays, one inch and a half beyond the red ones, measured upon their projection on a horizontal plane. I have no doubt but that their efficacy may be traced still somewhat further.

The fifth and sixth experiments show that the power of heating is extended to the utmost limits of the visible violet rays, but not beyond them; and that it is gradually impaired as the rays grow more refrangible.

The four last experiments prove that the maximum of the heating power is vested among the invisible rays, and is probably not less than half an inch beyond the last visible ones when projected in the manner before mentioned. The same experiments also show, that the sun's invisible rays, in their less refrangible state, and considerably beyond the maximum, still exert a heating power fully equal to that of red-coloured light; and that, consequently, if we may infer the quantity of the efficient from the effect produced, the invisible rays of the sun probably far exceed the visible ones in number.

To conclude, if we call *light*, those rays which illuminate objects, and *radiant heat*, those which heat bodies, it may be inquired, Whether light be essentially different from radiant heat? In answer to which I would suggest, that we are not allowed, by the rules of philosophising, to admit two different causes to explain certain effects, if they may be accounted for by one. A beam of radiant heat, emanating  
from



from the sun, consists of rays that are differently refrangible. The range of their extent, when dispersed by a prism, begins at violet-coloured light, where they are most refracted, and have the least efficacy. We have traced these calorific rays throughout the whole extent of the prismatic spectrum, and found their power increasing, while their refrangibility was lessened, as far as to the confines of red-coloured light. But their diminishing refrangibility, and increasing power, did not stop here; for we have pursued them a considerable way beyond the *prismatic spectrum*, into an invisible state, still exerting their increasing energy, with a decrease of refrangibility, up to the maximum of their power; and have also traced them to that state where, though still less refracted, their energy, on account, we may suppose, of their now failing density, decreased pretty fast; after which, the invisible *thermometrical spectrum*, if I may so call it, soon vanished.

If this be a true account of solar heat, for the support of which I appeal to my experiments, it remains only for us to admit, that such of the rays of the sun as have the refrangibility of those which are contained in the prismatic spectrum, by the construction of the organs of sight, are admitted under the appearance of light and colours; and that the rest, being stopped in the coats and humours of the eye, act upon them, as they are known to do upon all the other parts of our body, by occasioning a sensation of heat.

Slough, near Windsor,

March 17, 1800.

*Explanation of Plate I. fig. 2.*

AB, the small stand. 1, 2, 3, the thermometers upon it. CD, the prism at the window. E, the spectrum, thrown upon the table so as to bring the last quarter of an inch of the red colour upon the stand.

III. *Experiments on the solar and on the terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different.* By WILLIAM HERSCHEL, LL.D. F.R.S.\*

Part I.

THE word *heat*, in its most common acceptation, denotes a certain sensation, which is well known to every person. The cause of this sensation, to avoid ambiguity, ought to have been distinguished by a name different from that which is used to point out its effect. Various authors, indeed, who have treated on the subject of heat, have occasionally added certain terms to distinguish their conceptions, such as, latent, absolute, specific, sensible heat; while others have adopted the new expressions of caloric, and the matter of heat. None of these descriptive appellations, however, would have completely answered my purpose. I might, as in the preceding papers, have used the name radiant heat, which has been introduced by a celebrated author, and which certainly is not very different from the expressions I have now adopted; but, by calling the subject of my researches, the rays that occasion heat, I cannot be misunderstood as meaning that these rays themselves are heat; nor do I in any respect engage myself to show in what manner they produce heat.

From what has been said, it follows that any objections that may be alleged from the supposed agency of heat in other circumstances than in its state of radiance, or heat-making rays, cannot be admitted against my experiments. For, notwithstanding I may be inclined to believe that all phænomena in which heat is concerned, such as the expansion of bodies, fluidity, congelation, fermentation, friction, &c. as well as heat, in its various states of being latent, specific, absolute, or sensible, may be explained on the principle of heat-making rays, and vibrations occasioned by them in

\* From the *Transactions of the Royal Society* for 1800.



the parts of bodies ; yet this is not intended, at present, to be any part of what I shall endeavour to establish.

I must also remark that, in using the word *rays*, I do not mean to oppose, much less to countenance, the opinion of those philosophers who still believe that light itself comes to us from the sun, not by rays, but by the supposed vibrations of an elastic ether, every where diffused throughout space ; I only claim the same privilege for the rays that occasion heat, which they are willing to allow to those that illuminate objects. For, in what manner soever this radiance may be effected, it will be fully proved hereafter that the evidence either for rays, or for vibrations which occasion heat, stands on the same foundation on which the radiance of the illuminating principle, light, is built.

In order to enter on our subject with some regularity, it will be necessary to distinguish heat into six different kinds, three whereof are solar, and three terrestrial ; but, as the divisions of terrestrial heat strictly resemble those of solar, it will not be necessary to treat of them separately ; our subject, therefore, may be reduced to the three following general heads :

We shall begin with the heat of luminous bodies in general, such as, in the first place, we have it directly from the sun ; and as, in the second, we may obtain it from terrestrial flames, such as torches, candles, lamps, blue-lights, &c.

Our next division comprehends the heat of coloured radiants. This we obtain, in the first place, from the sun, by separating its rays in a prism ; and, in the second, by having recourse to culinary fires openly exposed.

The third division relates to heat obtained from radiants, where neither light nor colour in the rays can be perceived. This, as I have shown, is to be had, in the first place, directly from the sun, by means of a prism applied to its rays ; and, in the second, we may have it from fires inclosed in stoves, and from red-hot iron cooled till it can no longer be seen in the dark.

Besides the arrangement in the order of my experiments which would arise from this division, we have another subject to consider. For, since the chief design of this paper is to

give a comparative view of the operations that may be performed on the rays that occasion heat, and of those which we already know to have been effected on the rays that occasion light; it will be necessary to take a short review of the latter. I shall merely select such facts as not only are perfectly well known, but especially such as will answer the intention of my comparative view, and arrange them in the following order.

1. Light, both solar and terrestrial, is a sensation occasioned by rays emanating from luminous bodies, which have a power of illuminating objects; and, according to circumstances, of making them appear of various colours.

2. These rays are subject to the laws of reflection.

3. They are likewise subject to the laws of refraction.

4. They are of different refrangibility.

5. They are liable to be stopped, in certain proportions, when transmitted through diaphanous bodies.

6. They are liable to be scattered on rough surfaces.

7. They have hitherto been supposed to have a power of heating bodies; but this remains to be examined.

The similar propositions relating to heat, which are intended to be proved in this paper, will stand as follows:

1. Heat, both solar and terrestrial, is a sensation occasioned by rays emanating from candent substances, which have a power of heating bodies.

2. These rays are subject to the laws of reflection.

3. They are likewise subject to the laws of refraction.

4. They are of different refrangibility.

5. They are liable to be stopped, in certain proportions, when transmitted through diaphanous bodies.

6. They are liable to be scattered on rough surfaces.

7. They may be supposed, when in a certain state of energy, to have a power of illuminating objects; but this remains to be examined.

Before I can go on, I have to mention that the number of experiments which will be required to make good all these points, exceeds the usual length of my papers; on which account, I shall divide the present one into two parts. Proceeding, therefore, now to an investigation of the three first



heads that have been proposed, I reserve the three next, and a discussion which will be brought on by the seventh article, for the second part.

*1st Experiment. Reflection of the Heat of the Sun.*

I exposed the thermometer, which in a former paper has been denoted by No. 3, to the eye-end of a ten-feet Newtonian telescope, which carried a *Camera eye-piece*\*, but no eye-glass. When, by proper adjustment, the focus came to the ball of the thermometer, it rose from 52 degrees to 110; so that rays which came from the sun, underwent three regular reflections; one on a concave mirror, and the other two on two plain ones. Now these rays, whether they were those of light or not, for that our experiment cannot ascertain, had a power of occasioning heat, which was manifested in raising the thermometer 58 degrees.

*2d Experiment. Reflection of the Heat of a Candle.*

At the distance of 29 inches from a candle, I planted a small steel mirror, of  $3\frac{4}{5}$  inches diameter, and about  $2\frac{3}{4}$  inches focal length†. In the secondary focus of it, I placed the ball of the thermometer which in my paper has been marked No. 2; and very near it, but out of the reach of reflection, the thermometer No. 3. Having covered the mirror till both were come to the temperature of their stations, I began as follows:

	No. 2.	No. 3.	} Here, in five minutes, the thermometer No. 2, received $3\frac{1}{4}$ degrees of heat from the candle by reflected rays. I now covered the mirror, but left all the rest of the apparatus untouched.
	In the Focus.	Standard.	
0'	54	54	
1	55	54	
2	56	54	
3	57	54	
4	$57\frac{1}{4}$	54	
5	$57\frac{1}{4}$	54	
	No. 2.	No. 3.	} Here, in six minutes, the thermometer lost the $3\frac{1}{4}$ degrees of heat again, which it had gained before. I uncovered the mirror once more.
	In the Focus.	Standard.	
0'	$57\frac{1}{4}$	54	
1	$55\frac{1}{2}$	54	
$1\frac{1}{2}$	55	54	
6	54	54	

\* See Phil. Transf. Vol. LXXII. p. 176.

† See Plate I. fig. 1.

0'	54	54	} And, in five minutes, the $3\frac{1}{4}$ degrees of heat were regained. In consequence of which we are assured that certain rays came from the candle, subject to the laws of reflection, which, though they might not be the rays of light, for that our experiment does not determine, were evidently invested with a power of heating the thermometer placed in the focus of the mirror.
$1\frac{1}{2}$	56	54	
$3\frac{1}{2}$	57	54	
5	$57\frac{1}{4}$	54	

*3d Experiment. Reflection of the Heat that accompanies the Solar prismatic Colours.*

In the spectrum of the sun, given by a prism, I placed my small steel mirror, with a thermometer in its focus\*. It was covered by a piece of pasteboard, which, through a proper opening, admitted all the visible colours to fall on its polished surface, but excluded every other ray of heat that might be either on the violet or on the red side, beyond the spectrum. Then, placing the apparatus so as to have the thermometer in the red rays, but keeping the mirror covered up till the thermometer became settled, I found it stationary at  $58^{\circ}$ . Uncovering the mirror, I had as follows:

	No. 2.	} Here, in two minutes, the thermometer rose 35 degrees, by reflected heat. I covered the mirror again, and, in a few minutes, the thermometer exposed to the direct prismatic red came down to $58^{\circ}$ again. And thus the prismatic colours, if they are not themselves the heat-making rays, are at least accompanied by such as have a power of occasioning heat, and are liable to be regularly reflected.
0'	58	
2	93	

*4th Experiment. Reflection of the Heat of a red-hot Poker.*

I placed the small steel mirror at twelve inches from a red-hot poker, set with its heated end upwards, in a perpendicular position, and so elevated as to throw its rays on the mirror†. The thermometer No. 2, was placed in its secondary focus, and had a small pasteboard screen, to guard its ball from the direct heat of the poker.

\* See Plate II. fig. 1.

† See Plate I. fig. 1.



No. 2.

0' 54 $\frac{1}{2}$ 1 $\frac{1}{2}$  93

I covered the mirror.

3' 65

Here, in 1 $\frac{1}{2}$  minute, the thermometer rose 38 $\frac{1}{2}$  degrees by reflected rays; and, when the mirror was covered up, it fell in the next 1 $\frac{1}{2}$  minute, 28 degrees. On which account we cannot but allow that certain rays, whether it be those that shine or not, issue from an ignited poker, which are subject to the regular laws of reflection, and have a power of heating bodies.

[To be continued.]

IV. *Account of some interesting Experiments, performed at the London Philosophical Society, respecting the Effects of Heat, excited by a Stream of Oxygen Gas thrown upon ignited Charcoal, on a number of Gems and other refractory Substances submitted to its Action; with a Description of the Apparatus employed\*.*

IT is well known that no methods with which we are yet acquainted, are capable of exciting a degree of heat which is at all to be compared with that produced by the agents employed in the operations about to be described. The striking experiment of the deflagration of iron-wire in oxygen gas is frequently accompanied with phænomena, not hitherto attended to as they deserve to be, which furnish strong proofs in support of this assertion.

A receiver, or bell-glass, that will hold about two quarts, with an aperture at the top of about one inch in diameter (such as is now usually denominated "a deflagrating glass"), is charged with oxygen gas, in the usual manner, on the shelf of a pneumatic tub, the aperture being stopped with a cork, or, which is better, covered with a piece of slate, or other suitable substance, ground flat so as to lie closely on its edges. If the vessel be then removed from the shelf into an earthen plate, with a sufficient quantity of water in it to cover the edge of the receiver, and prevent the escape of the gas from

\* From the unpublished minutes of the Society. We are promised, from the same quarter, several other interesting communications.

below,

below, the phænomena will be the better observed. The wire being prepared by coiling it up into a spiral, like a cork-screw, inserting its superior extremity into a cork or cover, adapted to the superior aperture of the receiver, and winding a little cotton round its lower extremity; the cotton is to be very slightly dipped in the melted tallow of a candle, and then set on fire. This gives such a degree of temperature to the wire as occasions it to take fire when introduced into the gas, which is done by removing the cork or cover from the receiver and substituting that containing the wire at the moment of its ignition.

The wire then begins to deflagrate, and little globules of melted iron are formed at its extremity; each falling off in succession when it acquires a weight sufficient to detach it from the remainder of the burning metal. One circumstance, which cannot fail to impress the mind strongly with the intensity of the heat produced in this experiment, is, that many of these globules, though they must necessarily first pass through the water in the plate, are always found, even when the water is two inches or upwards in depth within the receiver, to have fused the vitreous glazing completely through, and to be so strongly united with it, that they cannot be afterwards separated without detaching the glazing, together with the semi-vitrified and slag-like globule which adheres to it.

Another effect, which may be produced at pleasure, furnishes, if possible, a still more convincing proof of the intensity of the heat. The drops of liquid iron generally acquire the size of a small pea before they detach themselves. If the experimenter, watching the moment, give a quick jerk to the wire, and make the globule strike the side of the glass, it will often be found to melt its way through in an instant, leaving a hole in the glass, whose edges are as well defined as if it had been drilled. The glass used on this occasion in the Society, and which is such a one as Mr. Varley, the experimenter to this institution, makes use of in his public lectures, is generally as thick as a strong drinking tumbler; and it is worthy of remark, that, though in charging the glass, perhaps not a minute before the experiment, it



It must necessarily be immersed in water, neither its coldness or humidity hinders the passage of the globule, which seems to make its way through with as much ease as it would through a piece of writing paper. When the jerk is less powerful, the ball of iron will sometimes not go quite through, but lodge itself in the substance of the glass, which in that case generally softens so as to raise a protuberance on the outside.

The contemplation of such powerful effects of heat as these, induced the Society, on the suggestion of a very worthy member, and zealous cultivator of the sciences, Mr. Francillon, to enter upon the present experiments, which were performed upon the different species of gems hereafter enumerated; for all of which the Society was indebted to the politeness of the gentleman who first proposed the subject, and from whose very valuable cabinet they were accordingly supplied.

The experiments were made in the presence of the Society at different meetings, from 24th September to 26th November 1798, in the order in which they are here mentioned; the present account being compiled from the original minutes taken by Mr. Turton, their secretary, with that degree of accuracy which usually characterises them, and which have again been compared with the gems in their present state.

The apparatus employed will be described at the end of the present communication.

### *Diamonds.*

I. The first stone subjected to experiment was a rose diamond of the weight of  $\frac{1}{64}$ ths of a carat. It was put upon a piece of compact charcoal in which a small excavation had been previously made, and the flame of a lamp, urged by a common blow-pipe, then thrown into the cavity. When the charcoal was ignited it was presented to another blow-pipe, supplied by the gasometer with an uninterrupted stream of oxygen gas.

The diamond, at the end of one minute and fifty seconds, was found to weigh only  $\frac{4}{64}$ ths, having lost  $\frac{1}{15}$ ths of its original

ginal weight. It had lost its transparency, figure, and position.

Bergman observes, that the diamond, urged by a very intense heat, contracts a sort of foot upon its surface: no appearance of this kind was observed on the present occasion.

The unconsumed portion of the diamond, which was recut after the experiment, now exhibits exactly the same species of lustre as before. It originally "drew colour," as jewellers term it; that is, it had a slight brown tinge throughout, somewhat like that of a smoked topaz, but very pale. It has still the same colour. Mr. Francillon, whose experience in matters of this nature is equal to that of most persons, made an observation on this circumstance which deserves to be mentioned. It seems that there are some diamonds which, although in themselves colourless, contain "fouls" or "specks," as they are called, which are cavities filled with red, yellow, or brown earths; and these colours, being reflected through the transparent substance of the stone when cut, give it the appearance of being itself coloured. When such stones are subjected to heat, the earth in these cavities turns black; the stone after this "plays colourless," and the defect is cured. Those which "draw colour," that is, which are themselves tinged, undergo no change by being heated.

II. Another diamond, which weighed  $\frac{1}{8}\frac{7}{4}$ ths of a carat, was subjected to heat excited by oxygen gas in the same manner as the former. At the end of 1' 53" it was withdrawn, and was found to have lost  $\frac{1}{8}\frac{5}{4}$ ths, or  $\frac{1}{1}\frac{5}{7}$ ths of its original weight. In a few seconds more, it would have entirely disappeared. It was indeed the intention of the Society to have produced this effect; but one of those fortunate circumstances which sometimes lead to considerable discoveries, presented on this occasion a phænomenon even more interesting than that of the total dissipation of the gem, though no less to have been expected. The diamond was at this time accidentally thrown from the charcoal, and *was clearly and distinctly seen to flame as a combustible body in its passage through the air.* The flame was of a blueish pearl colour, and nearly



nearly three quarters of an inch in length. This experiment has since been frequently repeated, and always succeeds in nearly the same manner.

This stone remains still in the state in which the experiment left it. Its figure and polish are quite gone, and it is studded all over with carbonaceous matter of a somewhat metallic brilliancy, which, on examination with a magnifier, appears to be plumbago, in union with the surface of the diamond.

*Amethysts.*

III. An amethyst, weighing  $1\frac{1}{16}$  carat, in 40'' broke into several pieces. At the end of 1' 55'' the fragments were withdrawn and again weighed, as there was reason to suspect that some of them had flown off. Some of them, weighing  $\frac{2}{8}\frac{3}{4}$ ths of a carat, were then taken and exposed to the heat for 1' 15'', when they became fused into one mass, which, on cooling, again broke into several pieces.

The colour of the stone is gone; the fragments are semi-transparent, and very much resemble fragments of crystals of a pure white salt; such, for instance, as purified nitre. The fused mass might perhaps have been preserved entire, if the precaution of cooling it slowly, by placing it under a muffle, or in a crucible, in a furnace, and suffering the fire to die away gradually, had been taken; but this was not attempted.

IV. Another amethyst, weighing half a carat, exposed to the heat for 5', was fused into a globule which is nearly spheroidal and pretty smooth, but full of small bubbles and cracks, similar to those in glass which is thrown, when heated, into water. The whole is colourless and transparent like flint glass, except a portion on one side, which, having probably been least heated, looks like white enamel. The stone lost  $\frac{1}{32}$  part of its weight.

*Sapphires.*

V. An oriental sapphire, weighing  $1\frac{5}{64}$  carat, though the heat was applied with caution, by exposing it first to the action of a lamp urged by a common blow-pipe, broke into three pieces, but seemed to have undergone no other change

at the end of one minute. The fragments were then subjected to the heat excited by a stream of oxygen gas thrown upon the charcoal for 2' 6''. Two of the pieces became colourless and have some resemblance to alum; the third retains, in some measure, its original colour. Their present weight is  $\frac{4}{6}\frac{4}{4}$ ths of a carat. They have all of them lost a considerable portion of their transparency, but have a rough polish, and retain their facets.

VI. Another sapphire, in the form of a parallelopipedon, weighing  $\frac{3}{6}\frac{3}{4}$ ths of a carat, being exposed to the strong heat for 2' 53'', had its angles rounded off, so that it became reduced to the form of a tamarind-stone, and lost its transparency. It is now a semi-transparent whitish mass, but still retaining some traces of its original colour. It is in part covered and interspersed with a kind of brown scoriæ very like copper bronze. The weight is the same as at first.

VII. Seven sapphires, weighing together  $\frac{4}{6}\frac{8}{4}$ ths of a carat, heated for 9' 55'', lost no weight, but became agglutinated together; the fused coat in which they are enveloped having the appearance of a whitish semi-transparent vitreous cement, through which the remainder of the stones are seen still retaining somewhat of their colour. One portion or face of the mass, containing two of them joined as already mentioned, was afterwards subjected to the lapidary's wheel: it took a good polish, and proved as hard as at first. The appearance of the different parts of the polished face, is the same as that of other similar parts of the mass.

#### *Oriental Topazes.*

VIII. A topaz, weighing  $\frac{2}{6}\frac{0}{4}$ ths of a carat, was exposed for 2' 19'' to a heat excited by oxygen gas, thrown upon the charcoal on which it lay, in two streams, by means of a double blow-pipe of a new construction, suggested and executed by Mr. Varley, and which will be described, along with the other parts of the apparatus, in its proper place. The stone entirely lost its colour, and now resembles a piece of borax. The fusion, if there was any, was extremely superficial, being only just sufficient to destroy the polish, the form and facets still remaining, with the angles a little obtunded. Its weight is the same as at first.



IX. Three more, weighing  $\frac{3\frac{1}{4}}{8}$ ths of a carat, heated for 4' 26'', became agglutinated together, but were found to have lost none of their weight. Being heated a second time for 4' 30'', they melted into a globule, which broke in cooling. The principal portion is roundish, and the fusion has evidently been complete: it has a brownish appearance on the fused surface, but is clearer in the fracture; the whole being somewhat like a piece of dirty camphor. The weight is a little increased.

X. A topaz, weighing  $\frac{3\frac{1}{4}}{8}$ ths of a carat, after being exposed to the heat for 4' 33'', was found to have lost  $\frac{3}{8}$ ths. It had, like the last, lost its colour, but still partly retains its original figure. One small surface was afterwards tried on the lapidary's wheel, and found to be as hard as before.

*Brazilian Topaz.*

XI. A Brazilian topaz, weighing  $\frac{3\frac{1}{4}}{8}$ ths of a carat, after being in the heat for 3' 26'', came out  $\frac{2\frac{2}{4}}{8}$ ths. It was perfectly fused, and had become of an opaque white, like a fused salt; such, for example, as *sal prunellæ*.

*Oriental Rubies.*

XII. One weighing  $\frac{2\frac{2}{4}}{8}$ ths of a carat, heated for 2' 9'', seems slightly and superficially fused: it is cracked, but the pieces have not separated. The weight and colour are the same as before. It is well known that rubies, when exposed to a common lamp and blow-pipe, become colourless, but recover their colour suddenly and instantly on cooling, when a little below a red heat. This ruby, after its exposure to the intense heat excited by the decomposition of the oxygen gas, resumed its colour as it would have done if it had been exposed to a lower heat.

XIII. Five rubies, weighing together half a carat, were exposed to the heat for 4' without losing any weight. The whole became agglutinated into a shapeless mass, in which the remains of the original stones now appear interspersed in a white vitreous cement. The colour of the smaller stones was changed whitish, but one larger portion was but little altered in this respect.

XIV. Eight more, weighing  $1\frac{6}{84}$  carat, heated for  $5' 8''$ , lost no weight: they were agglutinated, like the former, into a shapeless mass (which afterwards broke in two), with the same appearance of a whitish vitreous matter interposed. One small portion of the surface still retained a fine polish. Another part of it was afterwards polished by the lapidary: the stone was found to be as hard as before.

XV. To prove whether a longer exposure would not produce a more perfect fusion, seven more, which weighed together half a carat, were subjected to the heat for  $9' 2''$ , but without undergoing any other change than that of being sufficiently fused to be firmly agglutinated together by a white vitreous cement: the colour of the stones themselves remains, in a certain degree, unaltered.

XVI. Nine rubies, weighing  $\frac{48}{64}$ ths of a carat, were intended to be exposed for a still longer portion of time, but, after  $6' 10''$ , were observed to be in fusion. They were then withdrawn, and the mass weighed, but no loss of weight was perceptible. They now compose an opaque, dirty-looking, whitish mass, somewhat like melted camphor, and which has evidently been perfectly fused. A few small portions still retain somewhat of their original colour; but would probably, also, have become white or gray, by a longer continuance of the heat.

It is, perhaps, worthy of observation, that a happy accidental arrangement of the substances employed, will sometimes produce instantaneously more powerful effects than a longer exposure under other circumstances. This was the case in the present instance; for the weight of the rubies used in the last experiment, was to that of those employed in the preceding one, as 3 to 2, and yet, though the heat was applied for little more than 2-3ds of the time, the effects were much more striking.

Some remark may perhaps also be applicable with regard to the white vitreous substance into which the surfaces of the stones become changed. Is it probable that the alkali of the charcoal contributed to destroy the colour of the stones? or, Would a carbonaceous substance, containing no alkali, have exhibited with them the same phenomena in all respects?

This,



This, and other questions which will suggest themselves, can only be satisfactorily answered by experiment. It seems at least possible, however, that the results might be different if fusion could be effected under circumstances which should preclude the possibility of extraneous admixture. It is, perhaps, not altogether absurd to suppose that the mass might in that case entirely retain its original colour; and that at some future, and perhaps not a very distant period, small stones and fragments may be fused into larger masses possessing all the properties of the natural gems with as much certainty as metals are now cast into ingots.

[To be continued.]

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V. *An Essay on the Declivities of Mountains.* By RICHARD KIRWAN, Esq. LL.D. F.R.S. and President of the Royal Irish Academy\*.

AMONG the various causes to whose activity the planet we inhabit owes its present wonderfully diversified appearance, some undoubtedly exerted their influence from its very origin, and others at subsequent periods; of these last, one at least, namely, the Noachian deluge, was universal in its operation, while the effects of many more were partial and local, such as those resulting from earthquakes, volcanos, particular inundations, &c.

In a general survey of the globe it is only to general causes whose operation was universal that our attention can be directed, the effects of partial causes being the proper objects of the geological history of those countries that were particularly affected by them.

But to distinguish causes of the former class from those whose operation was more confined, it is necessary to discover some character by which their effects may unequivocally be discerned.

Now, a general *uniformity*, or agreement in some particular circumstance, in every part of the globe, seems to be a sure test of the operation of some general cause. The discovery of uniform appearances is therefore of primary importance

\* From the *Transactions of the Royal Irish Academy* for 1800.

in geological researches. In the present essay I shall confine myself to the investigation of one instance of this sort, namely, the *inequality of declivity* which the sides or flanks of mountains exhibit, in every part of the globe hitherto examined, according to the points of the compass to which they face, and are exposed.

That one part of almost every high mountain or hill is steeper than another, could not have escaped the notice of any person who had traversed such mountains; but that Nature, in the formation of such declivities, had any regard to different aspects or points of the compass, seems to have been first remarked by the celebrated Swedish geologist Mr. Tilas, in the 22d Vol. of the Memoires of Stockholm for 1760\*. Neither Varenius, Lulolph, nor Buffon in his Natural History published in 1748, have noticed this remarkable circumstance.

The observation of Tilas, however, relates only to the extreme ends, and not to the flanks of mountains: with respect to the former, he remarked that the *steepest* declivity always faces that part of the country where the land lies lowest, and the *gentlest* that part of the country where the land lies highest; and that, in the southern and eastern parts of Sweden, they consequently face the east and south-east, and in the northern the west. The essential part of this observation extends therefore only to the general elevation or depression of the country, and not to the bearings of these declivities.

The discovery that the different declivities of the flanks of mountains bear an invariable relation to their different aspects, seems to have been first published by Mr. Bergman in his Physical Description of the Earth, of which the second edition appeared in 1773. He there remarked that, in mountains that extend from north to south, the western flank is the *steepest* and the eastern the *gentlest*: and that, in mountains which run east and west, the southern declivity is the steepest, and the northern the gentlest. Vol. II. § 187.

This assertion he grounds on the observations related in his first Vol. § 32, namely, That 1stly, in Scandinavia, in the Suevoberg mountains that run north and south, separating

\* See also Vol. XXV. Swed. Abhandl. p. 291, where Cronstedt explains some obscure parts of Tilas's observation.



Sweden from Norway, the western or Norwegian sides are the steepest, and the eastern or Swedish the most moderate; the verticality or steepness of the former being to that of the latter as 40 or 50 to 4 or 2\*.

2dly, That the Alps are steeper on their western and southern sides than on the eastern and northern.

3dly, That in America the Cordelieres are steeper on the western side, which faces the Pacific Ocean, than on the eastern: but he does not notice a few exceptions to this rule in particular cases, which will hereafter be mentioned.

Buffon, in the first Volume of his *Epochs of Nature*, published in 1778, p. 185, is the next who notices the general prevalence of this phenomenon, as far as relates to the eastern and western sides of the mountains that extend from north to south, but he is silent with respect to the north and south sides of the mountains that run from east to west; nay, he does not seem to have a just comprehension of this phenomenon, for he considers it conjointly with the general dip of the regions in which these mountains exist. Thus he tells us, Vol. I. p. 185, that in all continents the general declivity, taking it from the summit of mountains, is always more rapid on the western than on the eastern side; thus the summit of the chain of the Cordelieres is much nearer to the western shores than to the eastern: the chain which divides the whole length of Africa, from the Cape of Good Hope to the Mountains of the Moon, is nearer, he says, to the western than to the eastern seas: of this, however, he must have been ignorant, as that tract of country is still unknown.

The mountains which run from Cape Comorin through the peninsula of India, are, he says, much nearer to the sea on the east than on the west: he probably meant the contrary, as the fact is evidently so; and so he states it in the second Volume, p. 295: the same, he tells us, may be observed in islands and peninsulas, and in mountains.

This remarkable circumstance of mountains was, notwithstanding, so little noticed, that in 1792, the author of an excellent account of the territory of Carlsbad in Bohemia, tells

\* The verticality of the sides is inversely as the length of the descent.



us he had made an observation, which he had never met with in any physical description of the earth, namely, that the southern declivity of all mountains was much steeper than the northern, which he proves by instancing the *Erzgebirge* of Saxony, the Pyrenees, the mountains of Switzerland, Savoy, Carinthia, Tyrole, Moravia, the Carpathian, and Mount Hæmus in Turkey. 2 Bergm. Jour. 1792, p. 385, in the note.

Herman in his *Geology*, published in 1797, p. 90, has at least partially mentioned this circumstance, for he says that the eastern declivities of all mountains are much gentler and more thickly covered with secondary strata, and to a greater height, than the western flanks, which he instances in the Swedish and Norwegian mountains, the Alps, the Caucasian, the Appennine and the Ouralian mountains; but the declivities, bearing a southern or northern aspect, he does not mention.

La Metherie, in the fourth Volume of his *Theory of the Earth*, of which the second edition appeared in 1797, a work which abounds in excellent observations), p. 381\*, produces numerous instances of the inequality of the eastern and western declivities, but scarce any of the northern and southern, whose difference he does not seem to have noticed; but he makes a remark, which I have not seen elsewhere, that the coasts of different countries present similar declivities.

With regard to eastern and western aspects, he thinks that a different law has obtained in Africa from that which has been observed in other countries; for in that vast peninsula he imagines the eastern declivities of mountains are the steepest, and the western the gentlest. Of this, however, he adduces no other proof but that the greatest rivers are found on the western side: this proof seems insufficient, as, if mountains be situated far inland, great rivers may flow indiscriminately from any side of them, and sometimes few rivers flow even from the side whose descent is most moderate, for instance, from the eastern side of the mountains of Syria: the Elbe and the Oder, two of the greatest rivers in Germany, take their course from the western sides, the first of the

\* It is to be regretted that he scarce ever quotes his authorities.



Bohemian and the other of the Moravian mountains, which yet are the steepest; many originate from lakes, as the Shannon with us; many take such a winding course, that, from a bare knowledge of the place of their disembogement, it is impossible to judge from what side of a mountain they issue, if from any; their course at most discovers the depression of the general level of the country.

In 1798, the celebrated traveller and circumnavigator, John Reinhold Foster, published a geological tract, which merits so much more attention as all the facts were either observed by himself, or related to him by the immediate observers. In this he states, as a fact universally observed, that the south and south-east sides of almost every mountain are steep, but that the north and north-west sides are gently covered, and connected, with secondary strata in which organic remains abound, which he illustrates by various instances, some of which have been already, and others will presently be, mentioned.

At present this fact attracts the greatest attention, being obviously connected with the original structure of the globe, and clearly proving that mountains are not mere fortuitous eruptions, unconnected with transactions on the surface of the earth, as has of late been confidently advanced.

I shall now state the principal observations relative to this object that have been made in different parts of the world:

*In Europe.*

1° The mountains that separate Sweden from Norway extend from north to south; their western sides are steep, and the eastern gentle. 1 Bergm. Erdé Beschreib. p. 157.

2° The Carpathian mountains run from east to west; their southern sides, towards Hungary, are steep; their northern, towards Poland, moderate. Foster, § 46.

3° Doctor Walker, professor of natural history at Edinburgh, observed that the coasts and hills of Scotland are steeper and higher on the western side than on the eastern. Jamison's Mineralogy of Shetland, p. 3.—However, Jamison observed that the south side of the isle of Arran is the lowest, and the north side the highest: p. 51.

4° The mountains of Wales are gentle on the eastern and steep on the western sides.

5° The mountains of Parthery, in the county of Mayo, are steep on the western side.

6° The mountains which separate Saxony from Bohemia descend gently on the Saxon or northern side, but are steep on the Bohemian or southern side. Charpent. p. 75.—The southern declivity is to the northern as six to two. 2d Bergm. Journ. 1792, p. 384 and 385.

7° The mountains which separate Silesia from Bohemia run nearly from east to west, yet are steeper on the northern or Silesian side than on the opposite Bohemian. Assmanni Silesia, p. 335. Such branches as run from north-east to south-west have their western covered with primordial strata, and consequently less steep. 4 New Roz. p. 157.

8° The Meissener in Hesse is steeper on the north and east sides, which face the Warra, than on the south and western. 1 Bergm. Journ. 1789, p. 272.

9° The mountains of the Hartz and Habichtswald are steep on the south and gentle on the northern sides. Foster, § 46.

10° The Pyrenees, which run from east to west, are steeper on the southern or Spanish side. Carbonieres XIII.

11° The mountains of Crim Tartary are gentle on the northern and steep on the southern sides. Foster, *ibid.*

### *In Asia.*

12° The Ourals, which stretch from north to south, are far steeper on the western than on the southern sides. Herman Geologie, p. 90; and 2d Ural Beschreib. p. 389.

13° The mountain of Armenie, to the west of the Ourals, is steep on its east and north sides, but gentle on the southern and western. 1 Pallas Voy. p. 277.

14° The Altaïshan mountains are steep on their southern and western sides, but gentle on the northern and eastern. Foster, *ibid.* and Herman 2d Ural Beschreib. p. 390 in the note.

15° So also are the mountains of Caucasus. 3d Schrift. Berl. Gesellsch. 471.

16° The



16° The mountains of Kamskatka are steep on the eastern sides. Pallas, 1 Act. Petropol. 1777, p. 43.

17° The Ghauts in the Indian peninsula are steep on the western sides.

18° The mountains of Syria, which run from north to south, skirting the Mediterranean, are said to be steeper on the western side facing the Mediterranean. 4 La Metherie, p. 380.

*In America.*

The Cordelieres run from north to south; their western flanks towards the Pacific are steep, their eastern descend gradually.

In Guiana there is a chain of mountains that run from east to west; their southern flanks are steep, their northern gentle. Voyages de Condamine, p. 140.

To assign the causes of this almost universal allotment of unequal declivities to opposite points, and why the greatest are directed to the west and south, and the gentlest, on the contrary, to the east and north, it is necessary to consider,

1° That all mountains were formed while covered with water.

2° That the earth was universally covered with water at two different æras, that of the Creation, and that of the Noachian deluge.

3° That in the first æra we must distinguish two different periods, that which preceded the appearance of dry land, and that which succeeded the creation of fish, but before the sea had been reduced nearly to its present level: during the former, the primæval mountains were formed, and during the last, most of the secondary mountains and strata were formed.

4° That all mountains extend either from east to west, or from north to south, or in some intermediate direction between these cardinal points, which need not be particularly mentioned here, as the same species of reasoning must be applied to them, as to those to whose aspect they approach most.

These preliminary circumstances being noticed, we are next to observe that, during the first æra, this vast mass of

water moved in two general directions, at right-angles with each other; the one from east to west, which needs not to be proved, being the course of tides which still continue, but were in that ocean necessarily stronger and higher than at present; the other from north to south; the water tending to those vast abysses then formed in the vicinity of the South Pole, as shown in my former essays. Before either motion could be propagated, a considerable time must have elapsed.

Now the primæval mountains formed at the commencement of the first æra, and before this double direction of the waters took place, must have opposed a considerable obstacle to the motion of that fluid in the sense that crossed that of the direction of these mountains. Thus, the mountains that stretch from north to south must have opposed the motion of the waters from east to west; this opposition, diminishing the motion of that fluid, disposed it to suffer the earthy particles, with which in those early periods it must have been impregnated, to crystallise or be deposited on these eastern flanks, and particularly on those of the highest mountains, for over the lower it could easily pass; these depositions, being incessantly repeated at heights gradually diminishing as the level of the waters gradually lowered, must have rendered the eastern declivities, or descent, gentle, gradual, and moderate; while the western sides, receiving no such accessions from depositions, must have remained steep and craggy.

Again, the primæval mountains that run from east to west, by opposing a similar resistance to the course of the waters from north to south, must have occasioned similar depositions on the northern sides of these mountains, against which these waters impinged, and thus smoothed them.

Where mountains intersect each other in an oblique direction, the north-east side of one range being contiguous to the south-west flanks of another range, there the afflux of adventitious particles on the north-east side of the one must have frequently extended to the south-west side of the other, particularly if that afflux were strong and copious; thus the *Erzgebirge* of Saxony, which run from west to east, have their north-east sides contiguous to the south-west side of the *Bohemian* *Erzgebirge* that separate Silesia from Bohemia; and hence these



these latter are covered with the same beds of gneiss, &c. as the northern sides of the Saxon, and thereby are rendered smooth and gentle comparatively to the opposite side, which, being sheltered, remains steep and abrupt; which explains the seventh observation.

The causes here assigned explain why the covering of adventitious strata on the highest mountains is generally thinnest at the greatest height, and thickest towards the foot, of the mountain; for the bulk of the water that contained the adventitious particles being proportioned to its depth, and the mass of earthy particles with which it was charged being proportioned to the bulk of water that contained them, it is plain, that as the height of water gradually decreased, the depositions from it on the higher parts of the mountains must have been less copious than on the lower, where they must have been oftener repeated.

Hence, 2°, granitic mountains, generally the most antient, frequently have their northern or eastern sides covered with strata of gneiss or micaceous schistus, and this often with argillite, or primæval sandstone or limestone, these being either of somewhat later formation, or longer suspendible in water.

Hence, 3°, different species of stone are often found at different heights of the same flank of a mountain, according as the water which conveyed these species happened to be differently impregnated at different heights: during the first æra its depositions formed the primitive stony masses; but after the creation of fish, limestone, sandstone, facrilites, and secondary argillites, in which piscine remains are found, were deposited. But, during the second æra, *viz.* that of the Noachian deluge, by reason of the violence and irregularity of its aggression, the depositions were more miscellaneous, and are found at the greatest heights; yet, in general, they may well be distinguished by the remains of land animals or of vegetables, or of both, which they present in their strata (or at least by the impressions of vegetables which they bear), as these must have been conveyed after the earth had been inhabited. But mountains regularly stratified bearing such re-  
mains,

main, for instance the carboniferous, cannot be deemed to have been formed in a period so tumultuous. During this deluge the waters also held a different course, proceeding at first from south to north, and afterwards in both opposite directions in our western hemisphere, as shown in treating of that catastrophe in my second essay.

Hence, and from various contingent local causes, as partial inundations, earthquakes, volcanoes, the erosion of rivers, the elapsion of strata, disintegration, the disruption of the lofty mounds by which many lakes were antiently hemmed in, several changes were produced in particular countries that may at first sight appear, though in reality they are not, exceptions to the operation of the general causes already stated.

Thus the mountains of Kamiskatka had their eastern flanks torn and rendered abrupt by the irruption of the general deluge, probably accompanied by earthquakes. And thus the Meissener had its east and north flanks undermined by the river Warra, as Werner has shown: thus the eighth and sixteenth observations are accounted for, as is the thirteenth, by the vast inundations so frequent in this country, 1 Pallas, p. 172, which undermined or corroded its east side, while the western were smoothed by the calcareous depositions from the numerous rivers in its vicinity.

Hence, 4<sup>o</sup>, we see why on different sides of lofty mountains different species of stones are found, as Pallas and Saussure have observed, 2 Sauss. § 981; a circumstance which Saussure imagined almost inexplicable, but which Dolomieu has since happily explained, by showing that the current which conveyed the calcareous substances to the northern, eastern, and north-eastern sides of the Alps, for instance, was stopped by the height of these mountains, and thus prevented from conveying them to the southern sides; and thus the north-eastern sides were rendered more gentle than the opposite. 3 New Roz. p. 425, conformably to the theory here given.

Hence, 5<sup>o</sup>, where several lofty ridges run parallel to each other it must frequently happen that the external should intercept the depositions that do not surmount them, and thus leave the internal ridges steep on both sides.

Hence,



Hence, 6°, low granitic or other primitive hills are frequently uncovered by adventitious strata on all sides, as at Phanet in the county of Donegal, or are covered on all sides; the impregnated waters either easily passing over them, or stagnating upon them, according to the greater or lesser rapidity of its course, and the obstacles it met with.

The two-fold motion of the antient ocean is noticed both by Buffon and Bergman, but neither of them has deduced from it the true explanation of the phenomena of which we here treat. Buffon attributes the formation of secondary mountains to deposition or sediments from the sea after the existence of fish, 1 *Epoques*, p. 143, in 8vo., which, he says, invested the bases of mountains, without noticing any distinction of sides, p. 144 and 170. He thinks these sediments were equally conveyed from both poles towards the equator; for it is in the equatorial regions that, he thinks, those mighty caverns opened, towards which the primitive ocean was impetuously borne, and in which it was ingulphed; p. 181, 182, and 183. If so, similar declivities should be formed on the southern as on the northern sides of mountains; which is contrary to the observed facts. His explanation of the eastern and western declivities is defective and erroneous; for he attributes the abruptness of the western sides to the erosion of the coasts on that side (an erosion that exists only in fancy), and the smoothness of the eastern, to the gradual desertion and retreat of the sea on that side, p. 184 and 185; a retreat equally fictitious, as De Luc has well shown. Whereas, since the general motion of the sea is from east to west, if the erosion were of either side, it should rather be on the eastern than on the western; besides, if the gentle declivities of the eastern sides of mountains arose from the gradual retreat of the sea, the petrifications of the secondary mountains thus formed should consist of such shell-fish as inhabit shallow seas or shores, whereas they consist chiefly of those called *pelagicæ*, which inhabit the greatest depths\*.

With respect to the eastern and western declivities, Mr. Bergman's account of the origin of their inequality agrees exactly with mine, 2 *Bergm. Erdeklotet*, § 183 and 187;

\* 2 *Bergm. Erdekugel*, p. 315.

but he fails in accounting for the inequality of the northern and southern, for he supposes the course of the waters to tend equally from both poles towards the equator; which would render the depositions equal on both sides; which is contrary to observation.

VI. *On the Identity of the Pyromucous, Pyrotartareous, and Pyroligneous Acids; and the Necessity of not considering them any longer as distinct Acids.* By C. FOURCROY and VAUQUELIN\*.

I. *On the Multiplicity of the Vegetable Acids.*

WHEN Bergman and Scheele had discovered several vegetable and animal acids before unknown, and had destroyed the pretended identity admitted in regard to those bodies, all the chemists employed themselves with great zeal in subjecting various vegetable acid substances to a new examination. The number of compounds, which till then had been reduced to two kinds, increased so much, that it was supposed it would far exceed that of the acids which had been discovered among the fossils, and that it would be impossible to set bounds to the multiplicity of these natural productions of organised bodies.

The four liquors, extracted by distillation from mucous bodies, wood, and tartar, though owing to an artificial decomposition, have been comprehended in the class of the vegetable acids, well characterised as possessing a peculiar nature: some analogies founded on their origin, their brown colour, and their burnt odour, have caused them to be referred to a special genus, under the name of the empyreumatic acids. If we collect the facts which seem to prove that they are different in nature not only from the other acids, but from each other, they will easily show that these acids have not been sufficiently examined to be well known; that chemists had only very imperfect ideas respecting their nature; and that it was for want of accurate analysis that

\* From the *Annales de Chimie*, No: 104.



they were thought to be different from each other, and different in their whole nature from any other vegetable acid.

## II. Characters given to the Pyromucous Acid.

Lemery, Boerhaave, Neumann, Cartheuser, Macquer, and Bucquet, were the first who insisted on the specific characters of that acid which has been called the pyromucous in the Methodical Nomenclature, and which had long been distinguished by the name of *spirit of boney*, of *sugar*, of *manna*, of *gum*, &c. M. Schrikel, in giving a detailed history of the analysis of sugar and of this acid in 1776, greatly enlarged and confirmed the ideas before conceived respecting the peculiar nature of the pyromucous acid, and all chemists have since adopted that opinion. It was characterised by its dark red colour when impure, and its golden yellow colour after being rectified; by its odour of caramel, its very pungent taste, its volatility, nearly similar to that of water; by its action on the skin, which it tinges red, and on the glass of retorts, which it seems to attack. It was believed to be entirely contained in mucous matters, and to be only developed by fire; it was said to be susceptible of decomposition by heat, and of being converted into gaseous bodies. But, besides that its supposed action on glass depended only on the adherence and penetration of the charcoal of the sugar; besides the proof, already well established, of its non-existence in saccharine and gummy matters, and of its entire production by fire, it must be allowed that, in examining what has been done in regard to saline compounds and the attractions of the pyromucous acid, no result had been obtained but vague and undetermined notions, which seemed to excite doubt and uncertainty in regard to the particular nature of this acid. The reader will be enabled to judge of the truth of this assertion by perusing, in particular, the details given under the article *Syrupy Acid* of the *Dictionnaire de Chimie Encyclopédique*, where our learned colleague Guyton has collected every thing then known respecting this empyreumatic acid: when drawing up, therefore, the article in my System of Chemical Knowledge, written and printed several months before the discovery now submitted to the Institute, I was

not able to obtain the real distinguishing characters of the pyromucous acid.

### III. *Characters ascribed to the Pyrotartareous Acid.*

We are indebted to Lemery and Neumann for our first knowledge of this acid, furnished by distilled tartar, and different from the tartareous acid. They found that tartar, by distillation, gave the fourth of its weight of acid phlegm. According to the prescription of Neumann, it was to be rectified at a gentle heat; but C. Guyton was not able to perform that operation with success, and the retort was always broken by the explosion which took place. The empyreumatic odour and taste of this acid are the two most striking characters which made it be admitted by chemists as a peculiar acid. It is difficult to conceive why Venel should take this acid product of distilled tartar to be nitric acid, which he thought might be extracted alone, and in a state of purity. It is no less astonishing to hear C. Monnet maintain, in consequence of experiments made at some length, that distilled acid of tartar is muriatic acid, though he really found in it, according to his own acknowledgement, only approximate characters. The academicians of Dijon and C. Berthollet, after repeating all the experiments of Monnet, were convinced that there is no real analogy between the acid product of distilled tartar and the muriatic acid. Scheele, in proving that there is always a little real muriatic acid in the alkali of tartar, insisted on the differences between this acid and that of tartar. M. Fontana proved that the acid distilled from tartar can be entirely resolved into carbonic acid gas and carbonated hydrogen gas; so that, by bringing it nearer to all the other vegetable acids, he removed further every idea of confounding it with the mineral acids. In the last place, Guyton, in the first part of the first volume of the *Dictionnaire de Chimie Encyclopédique*, published in 1786, after detailing all the different opinions as well as the principal facts known respecting this acid, concluded that it would be necessary to consider it as a peculiar acid; a product of tartar altered by heat, distinguished from the latter by its being incapable of crystallisation, and by the soluble salt which it



forms with lime; and he called it the tartareous empyreumatic acid. These ideas, adopted in 1787 by the authors of the Methodical Nomenclature, induced them to distinguish this acid by the name of the pyrotartareous acid. Since that period, chemists have continued to consider this acid as a distinct species, and no one has subjected it to a new examination.

#### IV. Characters given to the Pyroligneous Acid.

Boerhaave first made known, under the name of *acid spirit of wood*, the product distilled from woody bodies; and he even compared it to a kind of vinegar. M. Goetling has given a particular history of it in Crell's Chemical Journal for the year 1779. Guyton made it known afterwards, under the name of the ligneous acid, in the first part of the *Dictionnaire Encyclopédique*, published in 1786. He collected all the information then acquired respecting this acid, according to the experiments made by the Academy of Dijon. Every kind of wood examined has hitherto furnished by distillation the same acid; the difference in regard to colour, flavour, acrid odour, and particularly that of saline compounds, formed by the liquid distilled from wood, have made it to be distinguished from every other acid: in the Methodical Nomenclature, it has been called the pyroligneous acid, and in my System of Chemistry I classed it with the pyromucous and pyrotartareous acids, as forming with them a kind of acids composed of species analogous to each other in the ratio of their identic origin, and of some comparative properties manifestly depending on their empyreumatic state. The characters adopted to distinguish the pyro-ligneous acid were, its smoky smell, its reddish colour, its property of giving a lasting stain to wood, the salts which it formed with alkaline bases, and the elective attractions which it obeyed. In giving a systematic explanation of the properties of this acid, as well as in regard to those of the pyromucous and pyrotartareous acid, the want of more accurate knowledge was always felt; characters sufficiently striking and well defined, proper to distinguish these three acids from each other, could not be found; and even after they had been-classed together into a distinct genus, on account of their empyreumatic

matic nature, which gave them a striking resemblance, when chemists wished to explain what were the peculiarities of each species, they were obliged to acknowledge that the examination of these productions of fire had not been carried to a sufficient length, and that their properties had not been so thoroughly studied as to enable them to assign characters sufficiently striking, and to give a correct history of them.

V. *First View of the Acetous Nature of the Empyreumatic Acids.*

Such was the uncertain state of the science in regard to the three acids extracted from vegetable matters by fire, when a circumstance, presented, as it were, accidentally, gave us an opportunity of discovering a new fact respecting their nature. In the course of those researches in which C. Vauquelin and myself have been so long engaged respecting the chemical analysis of vegetable substances, and of the experiments which we made on the solid or ligneous parts of plants, we were employed some months in the examination of cork. This epidermal covering of a species of oak had appeared to me, for several years, to be of a nature so very distinct from other vegetable tissues as to deserve a particular research: several experiments on the epidermis of other trees had inclined me to think that in their characters they approached near to cork, and I thought it my duty to present it as one of the immediate materials of plants under the name of *suber*. Being desirous of carrying my first essays still further, and of giving more extent to these first ideas, we began to employ ourselves this summer on a more exact analysis of cork. Having subjected a pretty large quantity of it, 0.6 lb. (three hectogrammes), to distillation with a naked fire, we obtained from it a fourth of its weight of a very light and volatile oil, and almost the same quantity of a reddish empyreumatic liquid, which exhibited all the apparent characters of the pyroligneous acid. But this acid liquor, when examined with more care, rectified and combined with alkalies, and disengaged from its bases by weak sulphuric acid, soon exhibited properties of real acetous acid, and, after having separated it by slow distillation from the portion of coloured oil which



which it held in solution, it evidently appeared to be that acid.

When this fact was once ascertained, it was not difficult for us to presume that the pyroligneous acid, from whatever wood extracted, could only be acetous acid: we had even reason to suspect that the two other empyreumatic acids were of the same nature; but as a conjecture, however well founded, has no real value in physics, we called in the aid of experiment to enable us to discover the truth, or to reject our opinion as an error.

## VI. *New Examination of the Pyromucous, Pyrotartarcous, and Pyroligneous Acids.*

I. Having distilled, with great caution, sixteen parts of pure sugar, which furnished us with ten parts and a half of water charged with reddish pyromucous acid, a little more than four parts and a half of light charcoal, and half a part of gas, we combined this acid liquor with lime; the liquor was then evaporated to dryness, and afterwards treated in a retort with weak sulphuric acid. We thus obtained a pretty thick, clear, or very little coloured liquid, having a very sensible acetous smell, and which, when combined with potash, gave a very evident acetite of potash. This salt, indeed, had a dirty gray colour; but, by filtering a warm solution of it through pounded charcoal, it lost the oil which coloured it, and became quite clear.

The acid product, obtained from the first calcareous salt by the sulphuric acid, was already much less coloured than the pyromucous acid; it now retained only very faintly the smell of caramel (pan sugar), which characterises the acetous acid.

When disengaged a second time from alkaline salt by the sulphuric acid, it was still purer, had nothing of its first origin, and now exhaled only a pure acetous odour. We had reason, therefore, to conclude, from these experiments, that the pyromucous acid was nothing else than acetous acid united to an empyreumatic oil arising from the decomposition of the sugar by fire.

The same result took place with acid liquors extracted from

from gums, honey, manna, starch, paper, and several other vegetable substances which are known to yield pyromucous acid by distillation.

2. White and purified tartar gave by the retort nearly a half less of acid liquor, than sugar furnished of pyromucous acid. This tartareous acid product, obtained by a well regulated heat, was almost transparent, and had not the reddish-brown colour of the empyreumatic acids extracted by means of a strong heat. It however had an acrid empyreumatic odour, a heavy and highly coloured oil floated over it, and, notwithstanding its pungent nature, it would have been difficult to ascertain it to be acetous acid by this single experiment: but it did not long deceive us. Having saturated it with potash immediately, on account of its little colour and impurity, we distilled it, after being evaporated to dryness, with diluted sulphuric acid, and it furnished acetous acid, easy to be distinguished as such, without any mixture of empyreuma.

We found that by distilling pyrotartareous acid, even not rectified and in a red state, from off pounded charcoal, previously well lixiviated and carefully dried, it lost by this simple operation its empyreumatic nature: we are even persuaded that mere filtration of this acid warm, through charcoal, several times repeated, would be sufficient to separate from it the oil, and to bring it to the state of acetous acid almost pure.

No doubt, therefore, can remain, that the pyrotartareous acid is acetous acid rendered impure by a portion of empyreumatic oil produced by the decomposition of the tartareous acid by caloric.

3. The pyro-ligneous acid obtained from shavings of beech wood, distilled with care, formed with lime a brown salt not crystallisable. This salt, when treated with very weak sulphuric acid, gave acetous acid, known by its smell, the deliquescent acetite it formed with potash, or the crystallisable acetite obtained from it with soda.

Another portion of the same primitive pyroligneous acid, when immediately united to potash, and filtered in its solution through powdered charcoal, gave an acetite of potash almost colourless, from which weak sulphuric acid disengaged,

by



by distillation, very pure acetous acid, almost without empyreumatic odour or fumes.

It must here be observed that the pyroligneous acid, that of the three acids obtained by fire which has the most striking empyreumatic odour and character, is also one of the three most difficult to be purified, and to be carried to the state of very pure vinegar. It does not, therefore, assume the nature of the last, as the tartareous acid does, by filtration alone, nor even by two successive distillations from off powdered charcoal. By employing even the aid of ebullition and strong agitation with charcoal, it cannot be deprived of its oil, while both of these processes succeed with certainty and ease in purifying the pyromucous acid, which, indeed, gives up with most ease its oil; and even in purifying the pyrotartareous acid, though it be a little more difficult to separate its oil than that of the pyromucous acid.

But though it is more refractory in opposing purification, and that kind of analysis of which I here speak, it is no less proved, that the pyroligneous acid, like the two preceding, is only acetous acid impregnated with empyreumatic oil, produced from wood altered by heat.

#### VII. *Artificial Conversion of the pure Acetous Acid into the Pyromucous, Pyrotartareous, and Pyroligneous Acids.*

The preceding experiments might be sufficient to make us acquainted with the identic and real acetous nature of the three empyreumatic acids, which have been hitherto considered as forming three distinct acids, and belonging to a genus well characterised. But to these experiments a supplement was still wanting; it was necessary to inquire whether it was not possible to imitate these acids with that of vinegar, by adding what seemed to be added to it in each of these acids produced by fire.

It was before fully proved that each of these products of distillation differed from the other two by an empyreumatic oil united to it by the effect of its distillation alone. It was very easy, therefore, to try to distil vinegar from off oils distilled from a mucilage, tartar, and wood. This trial was attended with all the success that could be expected. Acid  
of

of vinegar, heated in a retort with each of these three oils, furnished a coloured and odorous product exactly of the same nature as primitive pyromucous, pyrotartareous, and pyroligneous acids: the odour and smell of these acids were very perceptible; only these factitious empyreumatic acids were a little stronger and sourer than those arising from distillation, but nothing more was necessary to carry them to the same state of weakness than to add to them a little water.

By repeated trial we obtained a manner of forming with the acid of vinegar the three empyreumatic acids much speedier, and in a more simple manner, than by distillation. We found that it was sufficient to throw some drops of the empyreumatic oil of mucilages, of tartar, or of wood, into the acetous acid, to suffer them to remain some time together, or to shake them strongly, to imitate these acids made by fire. The oil almost immediately unites with the acid, dissolves in it, gives it a red or brown colour, and communicates to it, at the same time, the acrid odour and bitter pungent taste known in the pyromucous, pyrotartareous, and pyroligneous acids.

The acetous acid is then a real solvent of empyreumatic oils, and in that case it assumes the characters of the acid liquors or odorous spirits, as they were formerly called, which issue from vegetable matters treated by fire. To separate these oils dissolved in the acetous acid, and to bring back the latter to its purity and to its primitive simplicity, the same means must be employed as for freeing the acetous acid from the empyreumatic acids united to it when obtained by distillation, *viz.* filtration, agitation, ebullition with charcoal, union with lime and alkalies, and the disengagement of these combinations by weak sulphuric acid.

It is to this solubility of oils in the acetous acid, that the odour which this acid so easily acquires and retains seems to be owing: it is to it that we must ascribe the distinction of a great number of empyreumatic acids, which several chemists have been induced to make, and which they could not avoid doing, while continuing to consider, in the acid products of distilled vegetable matters, the odour, colour, and flavour, as characters proper, if not for positively ascertaining,

at



at least for conjecturing them to be acids different from each other; and especially by comparing these characters in acid products furnished by kinds of wood very different in their properties, and consequently in their products.

VIII. *Reflections on the Frequency and the Nature of the different Processes which furnish Acetous Acid.*

After having proved that mucilages, saccharine bodies, fæculæ, as well as wood and tartareous compounds, give by distillation real acetous acid, concealed in the products of each of these bodies by a portion of oil, having a peculiar odour, smell, and colour, and that, consequently, we ought to deduct from the number of the vegetable acids these three empyreumatic acids, we thought it might be useful to science to give some observations here on the production of the acetous acid. The knowledge which the chemical art possesses at present respecting this production, though much more extensive than formerly, has never yet been collected into any work; it may, however, be of great importance in vegetable analysis: such is the motive which induced us to present a short view of it, without, however, entering into all the details which the subject might require in a systematic work.

The formation of the acetous acid, which has always been believed to be necessarily produced by a fermentation peculiar to vinous liquors, is so frequent in the processes of art and the operations of nature, that it is indispensably necessary to make it a general phenomenon, and to study this phenomenon under the name of *acetification*, as proper to throw light on the chemical properties of organised bodies. We may consider it either in regard to substances susceptible of acetification, or in regard to circumstances which favour transformation into acetous acid.

Under the first point of view, besides vinous liquors, which were first found, and long thought to be, the only ones capable of acetification, we have found a multitude of bodies susceptible of experiencing this change. There is scarcely any vegetable extract in which acetites are not found: sap, if it has been kept only a few hours, contains some of it;

even different kinds of mould itself are charged with it, as may be easily proved by distilling them with a little diluted sulphuric acid: tan, when heated, emits an odour of vinegar, and furnishes some of it by the same treatment: water in which pulse, cabbages, carrots, turnips, potatoes, cucumbers, the pods of French beans, &c. have been steeped and grown sour, is exceedingly acetous: the water from the starch manufactories is the same: the juices of acid fruits, those of apples, pears, gooseberries, strawberries, raspberries, cherries, oranges, and lemons, when exposed some hours to the warm air, assume, along with a strong and pungent odour, a taste differently and more strongly acid than that which they had naturally; besides perceiving in them acetous acid, you will obtain it pure and insulated by subjecting these juices to distillation. It is well known from the experiments of Scheele, that milk in becoming sour gives acetous acid; we have found that bouilli and animal jelly form this acid also: in a word, we have said in other memoirs that the urine of the mammiferae, and that of man in particular, had the property of becoming acetous, and gave a great quantity of very strong acid by distillation.

Thus the number of the substances susceptible of acetification is very considerable: extractive matter, mucilage, saccharine bodies, fæcula, and starch; even ligneous bodies, tan, the greater part of the primitive vegetable acids, the gelatin of animals, the caseous matter, and even *urée*, that body peculiar to urine, and which characterises it by its remarkable properties, all these products of vegetable and animal organisation and life are equally susceptible of acetification.

It is true, that the circumstances under which we have presented their conversion into acetous acid, seem all to belong to a fermentation, and that it might be thought that they follow a formation more or less striking or fugitive of vinous matter, but it remains for us to show that these matters may be acetified by phenomena or causes different from fermentation; and this fact is already proved by the acid productions of the distillation, which form the principal subject of this memoir. It is here seen that the action of fire really acetifies gums, mucilages, tartrites, wood. A know-  
ledge



ledge of the chemical phenomena exhibited by vegetable substances in regard to their acidification in general, enables us to observe and distinguish four circumstances which promote their acetification or conversion into acetous acid.

The first is, the decomposing action of the fire in distillation. Without here entering into more extensive details on this subject, which, when we consider the object of this memoir, are less necessary, we shall content ourselves with observing, that this action of caloric, by disuniting the constituent principles of vegetable matters, combines a part of them in such a manner as to give birth to acetous acid; and that this conversion is accompanied with the formation of water, the formation and disengagement of gaseous carbonic acid, and the precipitation of carbon in the state of charcoal.

The second mode of acetification of vegetable compounds is that arising from the action of powerful acids, and particularly of the sulphuric, the nitric, and the oxygenated muriatic on these compounds. This kind of production takes place in gums, sugar, extracts, and gelatin, when treated by acids; the greater part of the other vegetable acids, and even alcohol itself, often experience such a change by the decomposing influence of the acids above mentioned. While this kind of acetification takes place, there are formed also water and carbonic acid; sometimes carbon likewise is deposited. We must here add, that this acetification is the last term of vegetable acidification in general, since in treating acetous acid by the same decomposing action of the mineral acids, you destroy its acetous nature, and make it pass to the state of water and carbonic acid, as is the case in every vegetable decomposition carried to its *maximum*.

The third mode of acetification is the oldest known of the whole, and the only one formerly admitted: it is the acetous fermentation that converts all the different kinds of wine into vinegar: in this there is neither a precipitation of carbon, nor disengagement of carbonic acid. It is well known that it takes place in consequence of an absorption of the oxygen of the atmosphere, and that it supposes the preexistence of vinous liquors.

In the last place, we consider as the fourth and last mode

of acetification, a kind of peculiar fermentation, which does not require the presence of wine, which takes place in matters foreign to the nature of vinous liquors, and which has some relation to putrid decomposition. It is that observed in animal liquors abandoned to themselves, and particularly in urine.

Each of these modes of acetification, though equally giving birth to acetous acid, and supposing the same composition from which that acid arises, since it is the same in every case when it has been purified, admits, however, a difference in the products which accompany it. Each of the acetous acids arising from it, presents indeed a specific character proper for making it known and for indicating the source which has given birth to it.

Thus, 1st, The acetous acid obtained by fire is empyreumatic; it holds in solution an acrid oil, which gives it a peculiar odour, colour, and flavour.

2d, Factitious acetous acid, produced by the action of other acids, is characterised by the presence of the malic, or of the oxalic acid, formed at the same time as itself, and by its weakness, depending on the proportion of the water, which is also formed with the three preceding acids.

3d, The acetous acid arising from wine contains tartar, alcohol, and a colouring matter, which give it a peculiar character. It is, as has been said, a spirituous acid.

4th, In the last place, the acetous acid produced by putrid fermentation is always united, in whole or partially, to ammonia, which, like it, arises from this septic commotion.

But whatever may be the matters or new compounds united to the acetous acid formed under any of the four circumstances abovementioned, this acid, capable of being separated with greater or less ease from each of these substances, is always the same, and always similar to that extracted from sour wine by the help of distillation.

It must therefore be now admitted that the acetous acid is not the sole and necessary product of the fermentation of wine, and that its production, exceedingly frequent, is one of the most constant phenomena of vegetable and animal analysis.



VII. *Account of a fatal Accident which happened to a Traveller on the Glacier of Buet; with some Cautions to those who through Curiosity may visit the Mountains of Savoy, and particularly the Glaciers. By M. A. PICTET, Professor of Philosophy\*.*

A SENTIMENT of curiosity, exceedingly natural, induces travellers from all parts of Europe to visit Mont Blanc, the highest point of the old world, and to examine the surrounding glaciers. Since the memorable ascension of the learned historian of the Alps, these places have acquired a new degree of interest: the geologue, the mineralogist, and the mere amateur repair thither with avidity; and even women are amply indemnified for the fatigue of the journey by the pleasure arising from the view of objects entirely new to them, and by the amiable and friendly reception they meet with from the inhabitants. Every thing unites to make this excursion, which is attended with no real dangers, the usual object of all curious travellers who visit Geneva and its environs.

The more this journey presents attractions, the more it is of importance to make known the dangers to which travellers who undertake it may be exposed merely by imprudence or inattention. Our principal view is utility, and without doubt it is of utility to make known, in all places where this journal may circulate, dangers which are indeed great to those ignorant of them or who forgot them, but of little importance to those who are forewarned or cautioned to avoid them. Had we treated this subject some years ago, the fatal accident which lately took place, and to which we were almost witnesses, would not, perhaps, have happened. This reflection will not allow us to hesitate any longer. The experience I have acquired by travelling among the mountains, either when accompanying my illustrious friend Saussure, or in ten journeys undertaken to the glaciers of Chamouni in particular, will perhaps entitle me to some confidence from those whom I am desirous of saving from uneasiness or

\* From *Bibliothèque Britannique*, No. 112.

danger:

danger : I shall, however, refer to the end of this article the cautions in this respect which have been suggested to me by experience, and shall proceed to an account of that event which induced me to take up my pen on the present subject.

C. d'Eymar, præfect of Lemán, an enlightened lover of the arts, and a passionate admirer of the beauties of nature, having lately proposed to visit the glaciers of Chamouni, a canton which at present forms the eastern boundaries of the department under his administration, invited me to accompany him ; and I readily embraced his obliging offer. We set out on the 7th of August, and slept the first night at Sallenches, as travellers do in general.

Next morning, during our first hour's march, we met a young man on foot, accompanied by a peasant who was carrying a valise. We were struck with the melancholy and dejected air of this peasant. When we arrived at Servoz, three leagues from Sallenches, we learned from Deville, a very intelligent and experienced guide who attended us, that the morning of the day before, a stranger, the companion of the young man we had met, being with his friend and a guide on the glacier of Buet, had suddenly disappeared, at the distance of some paces before them, in a crevice of the glacier covered with snow, which had given way under his feet. When they reached the mouth of the crevice, the bottom of which they could not perceive, the two survivors called out a great number of times, but in vain, to their unfortunate companion swallowed up in the abyss ; and they did not quit the place till they had lost all hope of his safety. M. Zimpfen, the young man whom we met, when he arrived at Servoz, had given Deville a commission in writing, to endeavour, if possible, to discover the body of M. Eschen, his friend, and to cause it to be interred.

As scarcely twenty-four hours had elapsed since the event, the sensible mind of d'Eymar was struck with a ray of hope, and he immediately and officially enjoined Deville (for he hesitated, and not without reason) to furnish himself with the necessary means, to set out in the utmost haste, accompanied by such a number of men as he presumed might be necessary, and to give him an account of whatever should be the result,



result. It was at least a journey of nine hours from Servoz to the glacier in question; and as this glacier did not form any part of those we intended to visit, it was impossible for us to be informed of the issue of the proposed search till we passed Servoz on our return from Chamouni, to which we continued our route not without dejection.

It is with regret that I am obliged to omit the details of the stay we made in this interesting valley; they have a character which will always be imprinted in my remembrance, but they would be foreign to the present object. However, while the brave Deville and his companions are engaged with their enterprise, I shall venture to suspend the impatience of my readers for a few moments in order to make them acquainted with the glacier of Buet, and the motives which might induce travellers to visit it.

It is to Messrs. De Luc, the two brothers, that philosophers and naturalists are indebted for discovering the possibility of reaching this summit covered with eternal snow. It is an insulated mountain situated in front of the central chain to which Mont Blanc and its glaciers belong, and separated from them by a lower chain that runs in a parallel direction. This glacier may be seen from Geneva immediately on the left of the Môle. It appears under the form of a ridge not very salient, and which seems to be easy of access. Messrs. De Luc were, however, deceived in this respect, and the account of three attempts they made to reach it, the last of which alone, on the 20th of September 1777, was attended with success, is one of the most interesting episodes to be found in the works of any naturalist. It was a desire to discover the law which the decrease of heat in boiling water follows in proportion as it is raised in the atmosphere, that induced them to visit this mountain, and to brave, three different times, difficulties and dangers of various kinds to which they were exposed in attempting to reach a summit that may be considered as the utmost possible boundary of philosophical observation. Honour to that science which inspires courage so persevering, and which produces it in succeeding generations! Our daring countrymen never suspected that, seventeen years after this expedition, Saussure would

would repeat their experiment on Mont Blanc itself, that is to say, about 850 toises higher than they were able to ascend after great danger and fatigue.

Some years ago, a much easier route for arriving at the summit of Buet or La Mortine (for it is known by that name also) than that followed by Messrs. De Luc was discovered, and by this route I ascended it twice without experiencing any difficulty. You first sleep at Chalets de Villy, the last place of pasture of the valley which begins at Servoz and terminates at the glacier of Buet. From Villy you proceed to the Col de Salenton by a path practicable for mules; you then encounter the mountain on its eastern and southern sides, and, passing alternate declivities of snow and slate, reach the summit at the end of two hours and a half. The mean of two observations of the barometer which I made there, and which were very little different in their results, gave me 1594 toises for its height above the level of the sea.

The mountain itself presents nothing very interesting in a lithologic point of view; it consists of slate intermixed with veins of rotten quartz, or quartz similar to stalactites; but as a belvidere, nothing in my opinion exists that can be compared to it. You here embrace at one view the whole space comprehended between Jura on the west, as far as the mouths of the Rhone on the east, and, on account of this circumstance alone, I considered this mountain as exceedingly proper for receiving signals in an intended measurement of a degree of latitude and two degrees of longitude in the parallel of Geneva; a plan which may be found detailed in a memoir published in the Philosophical Transactions of the Royal Society of London for the year 1791\*.

In

\* I cannot convey a better idea of the situation of this spot than by quoting the words of M. De Luc:—"It is difficult to give a description in words when they do not awaken sensations which have been felt; I do not, therefore, flatter myself with the hope of exciting in the minds of my readers those which I then experienced. The most profound silence prevailed in these regions; we perceived that they were not made for living beings; they were as little known to our guides as to us. The Champis goats never approach them, and consequently no hunter had ever ascended so far.

" This



In my second journey to mount Buet, I was under the disagreeable necessity of being constantly enveloped by clouds

“ This sensation of profound solitude was one of those which we could most easily explain, but it does not explain our situation. We were on an immense extent of snow, the whiteness of which nothing altered. The rays of the sun, reflected in a straight line from the snow towards that luminary, showed us how smooth it was; and this smoothness the imagination extended to every thing around. We saw nothing but this snow, and the heavens, towards which it was terminated in various folds, delicately rounded like those beautiful argentine clouds which are sometimes seen floating majestically in the pure atmosphere. This was exactly what produced the extraordinary sensation which we then experienced. We actually thought ourselves suspended in the air on one of these clouds.—And what kind of air? Never had we before seen it of such a colour. It was blue, dark and bright at the same time, which produced an inexpressible sensation of immensity.

“ It was near noon when we arrived; and, on raising our heads above the veil which so long concealed from us the eastern part of the horizon, we suddenly discovered the immense chain of the Alps in an extent of more than fifty leagues. On whatever side we turned our eyes, the whole horizon was covered with mountains. Its boundary on the west was nothing but the thickness of the air; for we overlooked the chain of Jura, distant about thirteen or fourteen leagues, so much that we could have seen the plains of Franche-Comté and Burgundy, if the air had been sufficiently transparent. On the south-west our view extended as far as Mount Cenis, and on the north-east probably as far as Saint Gothard. We were far raised above all the defiles of the Alps, and only a few of their peaks were elevated above us.

“ In all this vast space, where mountains were accumulated on each other, we could discover no plain but in a small corner to the west, the middle of which was occupied by Geneva; and on the north-east we saw, almost from one end to the other, the large valley through which the Rhone flows, from the place where it falls from the mountains, as far as Sion, the capital of the Valais, distant from the place where we were nine or ten leagues. All the rest was filled with mountains.

“ The details as well as the *ensemble* wou'd have excited the attention of the most indifferent beholder. A single view of the immense quantity of ice and snow which covers the Alps will be sufficient to satisfy the spectator respecting the duration of the Rhone, the Rhine, the Po, and the Danube. It inspires us with an idea that this is their common reservoir, and that it is sufficient to supply them with water during several years of drought. We compared, without having need of calculation, their streams with their sources. These sources appeared to us only small

clouds during the six hours I remained on it. I was exceedingly cold, and, in order to warm ourselves, I and my guides were obliged to construct a hut on the ridge of rocks nearest the summit. We had at hand large pieces of slate, and our building was so solid that it still exists, and has sheltered more than one curious traveller from the severity of the weather.

The glacier, which covers this summit, differs from the greater part of those accumulations of ice known under that name in this respect, that the latter generally occupy the valleys, or the defiles in which the ice has not been originally formed, but to which it has sunk down by its own weight, and the pressure of the ice above it; whereas the ice of Buet has been formed in the place where it exists; and at that height in our parallel the snow never melts in summer. This mountain, therefore, may serve to determine with some precision the lowest boundary of the snow in our climates.

Thus, for example, by observing from Geneva, through a telescope furnished with a micrometer, the vertical angle comprehended between the summit of the glacier and the lowest boundary of the snow, I found it to be  $16^{\circ} 14'$ , which, taking the distance of Buet from Geneva at 29820 toises, corresponds to 141 toises, the distance of this boundary below the summit, which places it at 1453 toises above the level of the sea.

It may here be asked, what is the mean annual temperature at this height in our latitude? We have pointed out somewhere in our Journal a very simple formula, which Saussure deduced from a great number of observations, and which represents very well the law of the decrement of the mean heat of the atmosphere from the bottom upwards. This decrement is a hundredth part of a degree of the thermometer of Reaumur per toise of perpendicular elevation. This for-

rills, when compared with the valleys filled with ice, from which they flowed. Mont Blanc, which rose above these valleys, seemed capable of furnishing alone, for a long time, a sufficiency of water for a river, so much was it loaded with ice from the top to the bottom; that is to say throughout a prodigious extent. — *Recherches sur les Modifications de l'Atmosphere*, vol. ii. p. 390.

mula,



mula, therefore, applied to the mean temperature at the level of the sea, in the parallel of  $46^{\circ}$  (the latitude of Buet), as established in Dr. Kirwan's Work on the Temperature of the Globe, viz.  $56.4^{\circ}$  of Fahrenheit, or  $10^{\circ},8$  of Reaumur, will give for 1453 toises  $14.5\bar{3}$  degrees to be deducted; which makes the mean annual temperature of the lower boundary of the snow, in that parallel, to be  $3.63$  degrees below zero\*.

It is not surprising, therefore, that this mountain should be crowned with a glacier, since the snow which falls there during the cold season, never entirely melts in the summer. The water produced by the partial melting of the surface of the snow filters through the porous snow beneath it, and freezing in its interstices gradually converts it into ice. In this manner is formed an accumulation, the thickness of which M. De Luc endeavoured to estimate from the following observation :

“We judged,” said he, “by the position of these small rocks, about 200 feet lower than the highest part of the ice, that they formed a part of the real summit of the mountain. The whole mass above them was nothing but ice, in the form of a cone; cut through its axis, 200 feet in height, with a very extensive base, and resting on the im-

\* I had occasion not long ago to discuss this formula with a philosopher, who observed to me, that, according to its nature, it was impossible it could be correct; because the density of the air, an element on which the preservation of heat in the different strata of the atmosphere essentially depends, decreases in geometrical progression; while the heights in toises, which represent the temperatures, proceed in arithmetical progression. I admitted the justness of the observation, speaking mathematically; but, in a physical point of view, as the formula is composed of co-efficients, some of them unknown or inappreciable, which gives to the temperature an arithmetical progression, decreasing from the bottom upwards, it is no less true, that this formula, however deceptive it may be, represents the mean results of observations sufficiently well to be employed with convenience, when an approximative quantity only is necessary; and this was exactly the case. The question was the mean temperature of mount Saint Bernard, the philosopher was Bonaparte, and the discussion took place at table, and even in the apartment of the celebrated man whose theory and calculations I was endeavouring to defend.

menſe extent of permanent ice, which covers the whole declivity of the ſummit.”

But I learned, not without ſurpriſe mixed with horror, by the event, of which I am going to give an account, that this glacier, ſo often viſited by travellers, and which I twice traversed myſelf with perfect ſecurity, contains ſome of thoſe fiſſures, covered with ſnow, which renders others dangerous when the proper precautions are not employed.

During our ſtay at Chamouni, C. d'Eymar having heard that the guide, who accompanied the unfortunate Eſchen, reſided ſomewhere in the valley, ſent for him, to learn the particulars of the accident. When he arrived, deſpair was ſtill painted in his looks and whole countenance; but we learned nothing from him that we did not before know. He was a guide, ſelected by chance, who ſeemed to be little acquainted with the mountains, and who, however, as he told us, had requeſted M. Eſchen, when they arrived at the glacier, not to ſeparate from his two companions. Hurried on, however, by that undeſcribable ſenſation which people ſometimes experience when they reach high ſummits, and obſerving at the top of the glacier, a little diſtance before him, two chamoy hunters, who were reſting themſelves, he hurried forwards to join them; and it was then that he diſappeared.

We ſhall now return to our narrative. As we paſſed Servoz on the morning of the third day, the body of the unfortunate Eſchen was conveyed thither. We viewed it with the liveliest emotion; and by minute inſpection we were convinced that he had not ſurvived his fall a ſingle moment. We were fully confirmed in this opinion by the details which were given to us, and by obſerving that three of the real ribs on each ſide were broken, and that there was a great depreſſion of the ſternum; ſymptoms which indicate that the unfortunate young man had experienced the moſt ſudden and moſt violent compreſſion. The body was no ways diſfigured, and his features, in perfect harmony, excited no idea of his having ſuffered pain. We learned by the paſſport found in his pockets, along with other articles, that his name was Frederick Auguſtus Eſchen; that he was born

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in the bishopric of Lubec, and that he was twenty-three years of age.

[To be concluded in the next Number.]

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VIII. *Memoir on the Ibis of the Antient Egyptians.* By  
C. CUVIER\*.

EVERY one has heard of the Ibis, a bird to which the antient Egyptians paid divine honours; images of which they placed in their temples; which they suffered to wander about unmolested in their cities; which they embalmed with as much care as they did their relations; to which they ascribed virginal purity, and an inviolable attachment to a country of which it was the emblem, and the figure of which the gods would have assumed, had they been forced to adopt one mortal.

No animal ought to have been so easy to be distinguished, for there is none of which they have left so many excellent descriptions, correct coloured figures, and even the birds themselves carefully preserved with their feathers, under the triple covering of that strong preserver bitumen, thick and close bandages, and of strong vases well covered with mastic. Yet among all the modern authors who have spoken of the ibis, there is none but Bruce, a traveller celebrated by his courage and his knowledge of natural history, who has avoided error in regard to the real species of this bird; and yet his ideas, however correct, have not been adopted by naturalists.

After several changes of opinion respecting the ibis, naturalists at present seem to agree in giving this appellation to a bird, a native of Africa, nearly of the size of the stork, with white plumage, and the wing feathers black; raised on long red legs, and armed with a long bent bill, sharp at the edges, indented at the extremity, round at its base, and of a pale yellow colour, and having its face covered with a yellow skin, destitute of feathers, and not extending beyond the eyes.

\* *Journal de Physique*, Fructidor, an. 8.

Such is the ibis of Perrault\*, the white ibis of Brisson†, the white Egyptian ibis of Buffon‡, the tantalus ibis of Linnæus in the twelfth edition of his works; such is the bird which in the National Musæum is called the Egyptian ibis, and which is there placed near, and not without reason, to the *curicaca* of Margrave, or the *tantalus loculator* of Linnæus, for they both have a hooked, sharp, and indented bill. To the same bird, according to Blumenbach, who, however, confesses that at present it is very rare, at least in lower Egypt, the Egyptians paid divine honours §.

I participated in the error of these celebrated men above mentioned, till I had an opportunity of examining myself some mummies of the ibis. This pleasure was afforded to me lately by Fourcroy, to whom General Grobert of the Artillery, on his return from Egypt, presented two of these mummies. On unwrapping it with care, we observed that the bones of the embalmed bird were much smaller than those of the tantalus; that they were scarcely so large as those of the curlew; that its beak resembled that of the latter, the length excepted, which was somewhat less, and not at all equal to the tantalus; in a word, that its plumage was white, with the wing feathers black, as mentioned by the antients.

We were therefore convinced, that the bird which the Egyptians embalmed was, by no means our tantalus ibis; that it was smaller; and that it was necessary to search for it among the genus of the curlew.

After some research, we found that the mummies of the

\* *Description d'un ibis blanc et de deux cicognes* in the Memoirs of the Acad. of Sciences, Vol. III. p. 61. Plate XIII. fig. 1. The bill is represented as truncated at the end; but this is the fault of the draftsman.

† *Numenius fordide albo rufescens, capite anteriore nudo rubro; lateribus rubro purpureo et carneo colore maculatis, remigibus majoribus, nigris, nectricibus fordide albo rufescentibus, rostro in exortu dilute luteo, in extremitate auranteo, pedibus griseis.*—*Ibis candida*. Brisson Ornith. Vol. V. p. 349.

‡ *Histoire des Animaux*, Vol. VIII. 4to. p. 14. Plate I. Planches ent. No. 389.

§ *Handbuch der Natur Geschichte*, p. 203. Edit. of 1782.



ibis, opened before our time by different naturalists, had been similar to ours. Buffon says expressly that he examined several of them; that the birds they contained had the bill and size of the curlew; and yet he blindly followed Perrault, in considering the African tantalus as the ibis. One of the mummies opened by Buffon still exists in the Museum; it is similar to those we have seen. Dr. Shaw, in the Supplement to his Travels\*, describes and gives an exact representation of the bones of a similar mummy; the bill, he says, was six inches English in length, and resembled that of the curlew, &c. In a word, his description agrees entirely with ours.

Caylus, in his *Recueil d'Antiquités* †, gives the figure of a mummy ibis, the height of which, with its bandages, was only one foot, seven inches, four lines; though he says expressly, that the bird was placed in it on its legs, with its head erect, and that no part was bent back in its embalmed state.

Hasselquist, who considered as the ibis a small white and black heron, assigns as his principal reason for doing so, that the height of this bird, which is equal to that of the crow, corresponds perfectly with the height of the mummies of the ibis ‡; how then could Linnæus give the name of ibis to a bird as large as the stork; how, in particular, could he consider this bird to be the same as the *ardea ibis* of Hasselquist, which, besides its small size, had a straight bill; and how could this last error, in regard to synonyms, be still retained in the *Systema Naturæ*?

The only figure of the bill of an embalmed ibis, which does not accord with ours, is that given by Edwards §. It is a third larger than it ought to be; but as it contradicts all the other testimonies, we must believe that it was taken

\* Edit. of Oxford, 1746. Plate V. p. 64—66.

† Vol. VI. Plate IX. fig. 1.

‡ Iter Palestinum, p. 249. Magnetudo gallinæ, seu cornicis; and p. 250, Vasa quæ in sepulchris inveniuntur, cum avibus conditis, hujus sunt magnitudinis.

§ Plate 105.

from the mummy of some bird different from the ibis; or that the draftsman enlarged the proportions \*.

It was necessary, therefore, to seek for the real ibis somewhere else than among these birds of the tantalus kind, of a great height and with a sharp bill. In viewing the collection of birds, which Lacepede has arranged in such beautiful order in the Musæum of Natural History, we discovered a species, never before mentioned or described in any of the systematic authors except Latham, and which alone corresponds to every thing indicated to us, by antient authors, monuments, and mummies, as characters of the ibis.

It is represented in the annexed figure (Plate III.); it is a bird of the size of the curlew; its bill is similar to that of the curlew, but proportionally a little shorter, and of a black colour; two-thirds of its head from the neck are bare of feathers, and covered with short black down; the plumage of the body, wings, and tail, is a dirty white, except the tips of the large wing feathers, which are black, and those of the lower part of the back, which are of the same colour, and, being long, fall over the tips of the wings when they are folded. The feet are black, and like those of the curlew.

The individual in question formed part of the stadtholder's collection, but we are unacquainted with its native country. Desmoulins, who has seen two others, assures us that they were both brought from Senegal; one of them was even brought from that country by Geoffroy de Villeneuve: but, besides that the climate of Senegal is in nothing different from that of the Nile, we shall see hereafter that Bruce found this species in abundance in Egypt; and I imagine that the moderns will not admit, without some latitude, the assertion of the antients, that the ibis never quitted that sacred land.

\* Since I read this memoir, C. Olivier had the goodness to show me the bones which he took from two mummies of the ibis, and to open with us two others. These bones were similar to those in the mummies of General Grobert: one of the four only were smaller, but it was easily observed by the epiphyses that they had belonged to a young individual. C. Olivier showed us also a bill two-thirds longer than those found commonly in mummies of the ibis; but this bill, however, was perfectly similar to that of the curlew; and in particular to that of the black ibis of Belon; but in no manner to that of the tantalus.

Besides,



Besides, this assertion would be as much contrary to the *tantalus ibis* as to our curlew; for the individuals which we possess of that species, have been brought from Senegal. It was from this country that Geoffroy de Villeneuve brought the one in the Musæum of Natural History: it is even much rarer in Egypt than our curlew, since, according to Perrault, no one had ever said that he saw it there, or had received it from that country.

Since we are now acquainted with this bird, if we examine the works of the antients, and antient monuments, we shall find every difficulty vanish, and that all their testimonies agree with the best of all, which is, the body of the bird itself preserved and embalmed. “The most common kind of the ibis,” says Herodotus \*, “has the head and neck bare, the plumage white, except the head, the neck, the tips of the wings, and the rump, which are black. The bill and feet resemble those of the other kind of ibis:” and he had said of the latter, “they are all black, have feet like the crane, and a hooked bill.” There are many modern travellers who do not give such good descriptions of the birds they observe as that which Herodotus has given of the ibis. How could naturalists apply this description to a bird having no part naked but the face, which is red? To a bird which has the rump white, and not black? This last character, however, was essential to the ibis. Plutarch says, in his Treatise *De Iside et Osiride*, that the manner in which the white was mixed with the black in the plumage of this bird, made it appear as if marked with a crescent; and, indeed, the union of the black of the rump with that of the two tips of the wings, which forms on the white a large semicircular indentation, makes the white have a resemblance to that figure.

It is more difficult to explain what he means when he says that the feet of the ibis form with its bill an equilateral triangle.

The paintings of Herculaneum †, however, place the matter beyond all doubt. Some of these paintings, which represent Egyptian ceremonies, exhibit several of these birds

\* Euterpe.

† Plates, No. 138 and 140 of the edition of David; and Vol. II. p. 335, No. 59; and p. 321, No. 60, of the original edition.

walking in the courts of the temples: they are perfectly similar to the bird which we have pointed out: we may in particular observe in them the characteristic blackness of the head and neck; and it may be readily seen, by comparing their size with that of the personages in the paintings, that it must have been a bird of about half a yard in height at most, and not a yard, like the *tantalus ibis*.

The mosaïc of Palestrine exhibits also in its middle part several ibises perched on buildings, and which are in nothing different from those of the paintings of Herculaneum. A *far-donyx* in the cabinet of Dr. Mead, copied by Dr. Shaw \*, and representing an ibis, seems to be a miniature of the bird which we are describing. A large bronze medal of Adrian, an engraving of which is given in the *Musæum de Farnese*, and another of the same emperor of silver †, exhibit figures of the ibis, which, notwithstanding their small size, have a great resemblance to the bird in question.

In regard to the figures sculptured on the plinth of the statue of the Nile at the Belvedere, and in the copy of it in the garden of the *Thuilleries*, they are not well enough finished to serve as proofs; but we still have a sufficient number to obviate every doubt.

We must do Bruce the justice to say that he discovered the real ibis ‡. His *abou-bannès*, compared with the bird we have described, is so similar, that it is hardly possible not to consider it as of the same species: and this traveller says expressly, that it seemed to him to resemble those contained in the repositories of the mummies. He says also, that this *abou-bannès*, or Father John, is very common on the banks of the Nile; while the bird represented by Buffon under the name of the white Egyptian ibis, was never seen there by him. This *abou-bannès* has been inserted by Latham, in his *Index Ornithologicus*, under the name of *Tantalus Æthiopicus*; but he does not speak of the conjecture of Bruce respecting its identity with the ibis.

\* Appendix, Plate V.

† *Musæum de Farnese*, Vol. VI. Plate XXVIII. fig. 6.; and Vol. III. Plate VI. fig. 11.

‡ Vol. V. p. 172, English edition.



All travellers who preceded or followed Bruce, seem to have been in an error. Belon \* gave the name of black Ibis to a bird, which is nothing else than a black curlew with a naked head, and the feet and bill red; which does not agree with the description of Herodotus, who says that the black ibis is black all over.

This bird of Belon is very common in collections; and yet as naturalists sought also in the black ibis a tantalus with a sharp bill, the modern naturalists have almost all said that Belon alone had seen this bird. Lacepede has rectified this error, and given the name of black ibis to the bird which had been distinguished by that name by Belon. In regard to the white ibis, Belon believed it was the stork; in which it is evident he contradicted every testimony: no one, therefore, has coincided in opinion with him in this point except the apothecaries, who have taken the stork for their emblem, because they have confounded it with the ibis, to which they ascribe the invention of injections.

Prosper Alpinus, who ascribes this invention to the ibis, gives no description of the bird in his work on the Medical Art of the Egyptians †. In his Natural History of Egypt ‡ he speaks only from the information of Herodotus, to whose account he only adds, I know not from what authority, that this bird in its figure and size resembles the stork. He says he had learned that both the black and the white kinds were found in abundance on the banks of the Nile; but it is evident that he did not believe that he had ever seen any of them.

Shaw, speaking of the ibis, says § that at present it is exceedingly rare, and that he had never seen it. His emseffy, or ox-bird, which Gmelin, very improperly, refers to the tantalus ibis, is of the size of the curlew, has a white body, and the feet and bill red. It frequents the meadows near cattle; its flesh is not well tasted, and soon spoils. It may be easily seen that this is not the tantalus, and still less the ibis, of the antients.

\* De la Nature des Oiseaux, Book IV. chap. 9, of the edition of 1555.

† De Medicina Ægyptiorum, Lib. I. fol. 1. Paris, 1646.

‡ Rerum Egypt. Lib. IV. cap. 1. Leyden, 1735.

§ Page 255, Emseffy ox-bird

Hasselquist was acquainted neither with the white nor the black ibis: his ardea ibis, is a small heron with a straight bill. Linnæus, in his tenth edition, placed it, very properly, among the herons; but he was wrong, as I have already said, in transporting it as a synonym of the genus of the tantalus.

Maillet, in his Description of Egypt\*, conjectures that the ibis may have been a bird peculiar to Egypt, which is called there Pharaoh's capon, and at Aleppo, *saphanbacha*. It devours serpents. Some of these birds are white, and some black and white, and it follows, more than a hundred leagues, the caravans which go from Cairo to Mecca, in order that it may feed on the carcases of the animals killed in the course of the journey, while in every other season none of them are seen on that route. But he does not consider this conjecture as certain. He even says that we must give over attempting to understand the antients when they speak in such a manner as if they had wished not to be understood; and he concludes with observing that the antients perhaps comprehended, indiscriminately, under the name of *ibis*, all the birds which were serviceable to Egypt in freeing it from those dangerous reptiles which the climate produces in abundance, such as the vulture, falcon, stork, kite, &c.

He was right in not considering his Pharaoh's capon as the ibis; for, though his description be very imperfect, and though Buffon thought he could distinguish in it the ibis, it may be readily seen, as well as by what Pococke says, that this bird must be carnivorous; and, indeed, we find by the figure which Bruce has given†, that Pharaoh's capon is nothing else than the *rachama*, or small white vulture with black wings, *vultur percnopterus*, Linn., a bird exceedingly different from that we have proved to be the ibis.

Pococke says that it appears, by the descriptions given of the ibis, and by the figures of it found in the temples of Upper Egypt, that it is a kind of crane. "I have seen," adds he, "a number of these birds in the islands of the Nile; they were, for the most part, grayish‡." These few

\* Part II. p. 23.

† Vol. V. p. 191.

‡ French translation of his Travels, 12mo. Vol. II. p. 53.



words are sufficient to prove that he was no better acquainted with the ibis than others.

The error which prevails at present in regard to the white ibis, began with Perrault; who is even the first person who described the *tantalus ibis* of the present time. This error, adopted by Brisson and Buffon, has passed into the twelfth edition of Linnæus, where it is confounded with that of Hasselquist, which had been inserted in the tenth, to form with it a compound altogether monstrous. It was founded on the very natural idea, that to devour serpents, a sharp bill, more or less analogous to that of the stork or heron, was necessary. This idea is even the only good objection that can be made against the identity of our bird and the ibis; for it may be said, How can a bird with a weak bill, such as the curlew, be able to devour these dangerous serpents?

But, besides that a reason of this kind cannot hold out against positive proofs, such as descriptions, figures, and mummies; besides that the serpents from which the ibis delivered Egypt were accounted exceedingly venomous, but not as of a large size; I could answer directly, that the mummified birds, which had a bill absolutely similar to that of our bird, were real devourers of serpents; for I found in one of their mummies the undigested remains of the skin and scales of serpents, which I presented to the Class. This destroys the objection that might be drawn from a passage of Cicero, where he gives to the ibis a corneous and strong bill: having never been in Egypt, he imagined, from simple analogy, that this must be really the case. Our European curlew, which has a bill still weaker than the ibis, devours eels, as I have been assured by an eye-witness.

I shall conclude this memoir with announcing the results. The *tantalus ibis* of Linnæus must remain a separate genus with the *tantalus locuator*. Their character will be *rostrum validum arcuatum, apice emarginatum*.

The other *tantalus* of the last editions ought to form a genus with the common curlew. We may give them the name of *numenius*. Their character will be, *rostrum gracile, arcuatum, apice inflatum*.

The ibis of the antients is not the ibis of Perrault and  
Buffon,

Buffon, which is a *tantalus*; nor the *ibis* of Haffelquist, which is an *ardea*; nor that of Maillet, which is a vulture; but a *numenius* or curlew, which has not yet been named by systematic authors, and of which Bruce has given a figure under the name of the *abou-bannès*. I call it *numenius ibis, albus, capite et collo nudis, remigibus pennis uropygii elongatis, rostro et pedibus nigris*.

The *tantalus ibis* of Linnæus, in the present state of his synonymy, comprehends four species, of three different genera:

A *tantalus*, the *ibis* of Perrault and Buffon;

An *ardea*, the *ibis* of Haffelquist;

Two *numenii*, the *ibis* of Belon;

And the ox-bird of Shaw.

The reader may judge by this instance, and many others, of the state in which the *Systema Naturæ* still remains, and of what importance it would be to free it gradually from those errors with which it abounds, and with which naturalists seem to load it still more, by accumulating, without choice and proper examination, species, characters, and synonyms.

IX. *A brief Examination of the received Doctrines respecting Heat or Caloric.* By ALEXANDER TILLOCH. Read before the Askefian Society, December 1799\*.

TO detail the various theories maintained at different periods, and applied to explain the phenomena to which heat gives rise in the numerous changes presented by Nature in chemical combinations and decompositions, would be taking up the time of this Society unnecessarily, as every member must be well acquainted with them. I shall therefore confine myself to the modern doctrines on this subject; and even with these I shall be brief, as my chief object is, to inquire, Whether none of the circumstances that accompany the known facts in this department of physics (such of them, I mean, as the limits I have prescribed to the present essay will

\* For a short account of this Society, see Note, p. 353, Vol. VII.



allow me to notice) have been overlooked? and whether, If all of them had been attended to, and the legitimate inferences been drawn from them, the theory now generally received would ever have been admitted into science?

The ground I am about to take may perhaps expose me to the danger of being considered as an innovator, or, what may be deemed worse by some, a sceptic as to certain opinions, rendered respectable by the great names that have embraced and maintained them; and at the same time extremely plausible from the apparent facility with which they are applied to explain many phenomena that daily present themselves to the eye of every attentive observer. My only answer is, that I am in search of truth; and so decided an enemy to mere theoretical speculations, that I neither admit myself, nor wish others to admit in physics what cannot be proved to be truth. When the human mind acquiesces on any ground short of this, it is either through misconception, indolence, or pusillanimity, than which nothing has tended more to retard science and shackle men with prejudices, leading them to receive great names for argument, and, for demonstration, long quotations. But, to proceed, I use the term *heat* to denote that substance which possesses those properties, is governed by those laws, and produces those effects which shall be immediately enumerated; and I prefer it to the term *caloric*\* for no other reason but because the latter is employed by many to denote heat existing in a certain state, in which, they say, it may be considered as having actually lost its original character: whereas I hold, and I even hope to convince the members of this Society, that it invariably retains the same character, properties, and mode of action.

\* The term *caloric* has been adopted in the new nomenclature to avoid that ambiguity and misconception which might, it is said, arise from employing the same term to express a substance, and the sensation produced by the action of that substance. By the same mode of reasoning, all the *substantives* should be changed in any language that has similar *verbs*. But suppose the argument for change, in the present instance, to have full force in some languages, it has little or none in regard to the English, which employs the word *warmth* to express the sensation occasioned by *heat*. I shall, however, use the terms *heat* and *caloric* indifferently.

(A) Heat

(A) Heat is diffused through all the bodies in nature; whether solids, liquids, or aëriform fluids.

(B) Heat tends to an equilibrium; so that, when by any means it is accumulated in particular substances, a portion is quickly given off to the surrounding bodies to bring the whole to one common temperature. On the other hand, where bodies have been deprived of a portion of it, heat is given off to them by, or heat passes to them from, the surrounding bodies, to restore the equilibrium.

(C) Solids, by the addition of heat, assume the form of liquids, and liquids the aëriform state. On the other hand, gases, by an abstraction of heat, become liquids; and liquids, solids.

(D) The dimensions of bodies are enlarged when heat is poured into them, and *vice versa*.

The laws we have enumerated are general, and the objections that may be stated against the truth of any of them so few, and so easily obviated, that they cannot affect any inference drawn from them. The apparent exceptions relate chiefly to the change of volume in some particular substances when passing from the liquid to the solid form, or the contrary. Water, for instance, in passing into the state of ice, assumes a larger volume, though heat is then passing out of it. The substance, however, is only apparently enlarged. In freezing, the water assumes a crystalline form; the crystals, shooting in every direction, crowd against, and, as it were, jostle each other, causing vacuities, which constitute no real part of the matter. We may compare this phenomenon to what takes place in a bundle of chips of wood, which will always, however closely packed, occupy an apparently larger space than the same weight of ligneous matter as arranged by nature in the tree. Another apparent exception may be noticed in clay, which diminishes the more in bulk the greater the quantity of heat poured into it: but here there is a misnomer—it is not clay, but *a mixture of clay and water* that is diminished in bulk. The water is driven off; and, where there is a diminution of matter, a reduction of volume must follow. Clay is therefore not an exception to  
the



the general law—drive all the water out of it, that is, convert it into glass, and it then obeys the general law.

We shall first attend a little to the operation of the foregoing laws in some given cases; and afterwards examine the doctrines which have been established from the phenomena observed to accompany them:

If two bodies of the same nature, unequally heated, be brought into contact, the heat will diffuse itself equally through them, and the quantities in each will bear the same proportion to each other as the masses themselves; but, if the bodies differ in kind, though equilibrium take place, and each indicate the same temperature, yet the proportions of heat in each will not be as the masses, but vary according as the bodies differ.

(E) This property of bodies to hold different quantities of heat, even when that fluid is in a state of equilibrium, Dr. Black calls *the capacity of a body for heat*; the quantity itself he calls *specific heat*. The distinction is perfectly philosophical, nor need the terms be objected to; but they should be accurately defined, that they may never be employed to convey ideas different from the facts that gave rise to their adoption.

That the sense in which we understand and use the terms may be clearly conceived, we shall substitute another substance for heat, all other circumstances being as before. If two substances of the same kind (two pieces of chalk), unequally wetted with water, be brought into contact, the one will give off water to the other (or the one will absorb water from the other) till the quantity in each be *as their masses*. If two bodies different in kind (chalk and wood) be thrown into water, the quantity they will each take in will be *as their capacities*: this will also be the case if, instead of throwing them into water, we suspend both in the same damp atmosphere; and the quantity in each, whether received by immersion in water or in the atmosphere, is *specific*. The latter term must be thus considered, otherwise we shall lead ourselves into error; for there is at least as great a difference between the extremes of the general temperature in summer and in winter as there is between a moist, and a comparatively dry (but yet moist) atmosphere.

Some philosophers tell us that *the cause* of this phenomenon of different bodies requiring different quantities of caloric to indicate the same temperature (or, in other words, the cause of their having different capacities for heat) arises from the different degrees of affinity which different bodies possess for heat! Is this an explanation?—Different bodies hold different quantities of heat, *because they have different capacities*:—Different bodies hold different quantities of heat, *because they have different affinities*!—yet we are to consider the one term as expressive of a *property*, and the other as expressive of *the cause* of that property! Is this consistent with that accuracy and precision which should prevail in the language of science?

But this is not all. Heat is considered as existing in two distinct states; free, and in chemical union. When heat is in equilibrio in any place; in other words, when a body is in equilibrio with the bodies which surround it with respect to its heat [that is, has received heat from the common stock proportioned to its capacity]; that quantity which it contains is termed *latent heat* or *caloric*; *combined caloric*; *heat in chemical union*. We are told that in this state it is not perceptible by any external sign or organ of sense; that it does not affect the thermometer, but remains quiescent in those bodies of which it constitutes a principle; and that it is then, more or less, in a state of confinement.

Is the heat in any body termed *latent* because the body has not a capacity to receive more? No; for if, by any means, an addition be made to the common stock of a prescribed system of bodies, each individual substance will still provide lodging for a share of the added quantity, proportioned to its capacity, in relation to the capacity of the rest. It should be observed too, that, let the common temperature be what it may, if heat be in equilibrio, the portion in each body is, by some at least, held to be *latent*, in contradistinction to what they term *free heat*. Hence it follows, that the quantity of *latent heat* in a given body differs in different seasons of the year; for heat may be in equilibrio in any system of bodies in summer as well as in winter. An assemblage of different bodies, in equilibrio as to caloric, in



Jamaica, the quantity in the whole is *latent*, and in each *specific* and *latent*: the same holds with regard to a system of substances in England in the month of January. The heat in both cases is *latent*. If the one system of bodies could be transported to the other in a moment, and placed in contact with it, a portion of that heat which was *latent* in Jamaica, would be counted *sensible* heat in London! This is exactly where the doctrine lands us when viewed generally; and yet we are to consider heat as having two distinct modes of existence! But let us take a closer view of it:

When by the passing of a solid body into a fluid form the surrounding atmosphere is found to have lost heat, this heat, they say, has not been merely absorbed by the substance that has become fluid, but has become latent in it. When a liquid passes into the aëriform state, the same phenomenon accompanies the change, and also the same assertion; and the doctrine is meant to convey the idea that heat has not only been changing its place, but has itself undergone a change as to its state, its properties, and mode of action.

It is a generally received axiom, that no more causes should be admitted in physics than what are true and sufficient to account for the phenomena. Let us inquire whether this axiom be not violated in the cases stated, when recourse is had to the doctrine of latent, as distinguished from free, heat, to assist in explaining the phenomena.

The dimensions of bodies are enlarged when heat is poured into them (D). The primitive molecu<sup>l</sup><sub>æ</sub> (independent of the heat) of which the bodies are formed, are forced farther from each other by the interposed matter of heat. Continue the action of the same cause, their cohesion will be destroyed entirely (C), and they will be diffused through the predominating substance (heat), as the particles of saline substances diffuse themselves through water when dissolved in that liquid. In the case just put for illustration, who ever supposes the water to have undergone any change as to its essence or properties, whether directly poured upon the saline substance or furnished to it by the atmosphere.

In chemical combinations it is admitted that we cannot,

*a priori*, determine the properties of the compound from a previous knowledge of those of the ingredients. Every substance has, for one of its properties, *a capacity for heat*. If two be united chemically, the capacity of the compound for heat may not be that of the sum of the capacities of the ingredients for the same substance. Indeed it seldom or never turns out so. If by any process then we change the capacity of two or more bodies; or, by uniting them, produce a third, whose capacity for heat is greater than the sum of that of the ingredients, this new compound must demand from the common stock of heat diffused through the atmosphere and other neighbouring substances (A) an additional portion. Is there any thing in all this to lead us to the notion that the passage of heat from the atmosphere, or other contiguous bodies, into the new compound, is the operation of any other than the general law by which heat tends to an equilibrium among all the bodies in a system (B), and diffuses itself among them in proportion to their capacities for heat (E)? Is not the operation of these laws sufficient to explain the phenomena without the aid of any others?

But, besides, is it not a general fact, in all cases where a capacity for a greater quantity of heat is produced in a compound than the sum of the capacities of the ingredients, that the sum of the volume of the compound is greater than that of the ingredients? And is it not equally true, that, when ingredients, before in union, are found, on being separated, to have acquired capacities for heat, whose sum is greater than was that of the body in which the ingredients were united, the sum of the volume of the ingredients is greater than the volume of the compound was\*? Can it be correct to say that it neither affects our senses, nor is cognisable by any external sign! when we can see with our eyes its effect in the increase it has made in the volume of

\* At the moment of writing I do not recollect a single exception. In cases where crystallisation is concerned there may be some apparent ones, as in the case of water and ice; but these will not affect my argument. It is enough for me that *the effect* holds generally in the cases to which the doctrine of latent heat is applied—an effect perfectly conformable to the general law (D), and also *cognisable by our senses*.



the compound substance before us; or in the sum of the masses of two or more substances over that of the compound from which they were separated?

Again—When gases pass into the form of liquids, or liquids into the solid state, heat is found to pass from them into the atmosphere, “the heat that was latent in them before now becomes sensible.” Was it *not cognisable* before? It ought not to have been so when latent. I find, however, that the volume of the body from which it has been separated is now *less* than before; and this is exactly what ought to take place from the general laws, not only of heat but of matter—a substance, heat, has been separated from a body in which it formed a part of the mass or volume, and the remaining mass is diminished in bulk.

Let us, however, alter the mode of expression. A body, no matter by what means, is reduced into a less volume; it is so constituted as to admit of this effect being passed upon it; for one of its ingredients, heat, is a subtile fluid which may be dislodged; nay, will even run out, when the capacity of the body which held it is lessened. It is no way strange, in this case, that a fluid poured out of its recipient, and which, by its tendency to equilibrium, must diffuse itself through the surrounding bodies in proportion to their capacities to receive it, should make itself manifest to our sense of feeling, we being immersed at the moment in one of the neighbouring substances, the atmosphere, which affords a lodging for a portion of the dislodged heat, and serves to transport other portions of it to all the neighbouring bodies. Nor is it strange that a substance which, while it formed an ingredient in the body from which it has been removed, constituted also a part of its volume, should, when it passes into another body, mercury in a glass tube, cause an increase in its volume proportioned to the quantity that has passed in.

It constituted *sensible bulk* in the first body, and was *latent heat*; it constitutes *sensible bulk* in the second body also, but there it is *sensible heat*!! Is there no absurdity in all this?

Heat in those combinations, in which the term *latent* is employed, not cognisable by our senses! Is it not obvious  
from

from the preceding statement that this is a mistake? Before the heat passed out of its then combination it made a part of the volume which we contemplated with our eyes, and the proportion that this part bore to the whole might, in many cases, be determined by actual measurement. Is sensible heat, or rather heat in such combinations as it is in when that term is used, cognisable by any other means?

To me it would appear as correct, when, by any power, the capacity of a vessel filled with a liquid (water) is diminished, to say that the portion of liquid thereby ejected is set at liberty; meaning thereby that it has undergone an essential change as to its form, properties, or mode of existence; as to say, when the volume of a substance containing heat is abridged, and a proportionate quantity of the heat is dislodged, that the heat, so dislodged, has undergone any such change.

The fact in my opinion is, the substance in which the caloric resided has undergone the change, where there is a change; not the heat: or, if it be changed, the change is of that kind which passes upon a brick or any other body when we move it from one room to another. This, I think, must be pretty clear from what we have already stated; but I hope to make it still more so by examining a particular case or two which have been made to serve for a foundation, as it were, for the doctrine which I oppose.

[To be continued.]

## NEW PUBLICATIONS.

*An Account of the Irides or Coronæ which appear around, and contiguous to, the Bodies of the Sun, Moon, and other Luminous Objects.* Cadell and Davies, 1799. 46 pages. With One Copperplate.

**T**HIS treatise is by the author of the *Observations on the Inflexions of Light*, of which a short account was given in the *Philosophical Magazine* for September. In making these observations, that ingenious gentleman perceived that  
a ray



a ray of white light, passing between angular surfaces, such as the almost meeting edges of two knives, was liable to be parted by its bendings, separations, and other changes, into those differently coloured rays, which had been supposed by Newton to be the primogenial elements of pure white light. This observation is here applied to explain the formation of those *irides* or *coronæ*, consisting of many-coloured circles, which are seen contiguous to the bodies of the sun, moon, &c.

The order of the colours in these coronæ is as follows:  
 1. Next to the body of the luminary a circle of grayish black, grayish blue, or faint diluted white, altering as it recedes into—2. A strong lucid white of considerable breadth.  
 3. Slender rings of yellow and red. 4. A succession of rings of violet, blue, green, yellow, red. 5. Green, diluted yellow, red; diluted green, diluted red. This is the common order; but it is occasionally varied by slight irregularities.

Newton has made the only plausible attempt to account for these appearances. He supposes them to be produced by the rays of light falling upon globules of water, hail, or ice. He assumes that globules of ice or hail may, or actually do, exist in situations in the atmosphere in which they can receive the rays, so as to produce that analysis of the white light, and that distribution of the coloured rays which these coronæ exhibit. He conjectures that these coronæ in the atmosphere, surrounding the sun or moon, may be produced in the same manner in which a miniature appearance somewhat similar is occasioned by the reflexion of light from a lens quicksilvered on the back-side.

But we do not know particles of hail or ice always to exist in the atmosphere when these many-coloured irides or coronæ appear. The ordinary refraction of direct light, in its passage through globules of liquid water, could not produce these appearances. Newton's theory, though all that he postulates were granted, would not explain that particular distribution of these colours in the coronæ, by which intervals or vacant spaces are left between the different portions of the red and of the other colours. The iris, from a globule of crystal having quicksilver on its further side, is, according to Newton, produced, not by the reflexion of the main beam of  
 light,

light, but by a *faint light* irregularly scattered in every direction from that beam: and a light so *faint* would scarcely be equal to the production of the many-coloured irides round the sun, moon, &c. in the circumstances in which these appear; nor, indeed, could the globules of water fall under any light so faint, where the main beam to which they are exposed is broader than their whole surfaces.

On the other hand, the author of this essay has observed that the *arrangement* of the colours and orders of colours in the many-coloured irides of which he treats, is not only impossible by any refraction that can be supposed; but is *perfectly the same with the arrangement of the colours in the two sets of fringes into which a ray of white light, admitted into a dark chamber, and made to pass between two parallel edges, is naturally divided*. Each of these two sets of fringes, if taken outward from the central line, exhibits precisely the same arrangement of colours, as does the solar iris taken from its interior to its exterior extremity. He therefore concludes that, in the formation of the many-coloured irides, *the sun's irradiation, passing between the edges of contiguous drops of water in the atmosphere, is, by the inflecting attraction which these exercise upon it, separated into two sets of fringes, which fringes constitute the irides, having the body of the sun for their central line*. This is the author's theory.

It is at least admirably ingenious; more simple and plausible than that which it strives to supersede; and founded upon a better induction of unquestionable facts. We should hope that the general discussion of philosophers must finally establish it as scientific truth. The essay is elegantly and unaffectedly written.

*A Treatise on the Chemical History and Medical Powers of some of the most celebrated Mineral Waters, &c. By WILLIAM SAUNDERS, M.D. F.R.S., &c. Phillips, George-yard, Lombard-street. 8vo. 483 Pages.*

THE more accurate analysis of mineral waters by modern chemistry, cannot but lead national physicians to estimate more justly than before, the nature of their respective medicinal powers.



In the work now before us, Dr. Saunders applies, with considerable skill, the information of chemical analysis to assist in explaining the proper remedial uses of the waters of the most celebrated mineral springs in Britain and on the Continent.

His work begins with an account of the physical qualities and the chemical composition of water; and of its natural uses in the animal economy. He then examines, what extraneous substances are liable to exist in solution in water, and by what chemical tests or re-agents the presence of any of them may be ascertained. A judicious chapter follows next, concerning the nature, as more or less salutary, of the waters in common use.

After this preliminary matter he descends into a particular account of the chemical analysis and the medical uses of three-and-twenty different species of mineralised waters. These are, the waters of Malvern, Holywell, Bristol Hotwell, Matlock, Buxton, Bath, the Sea, Sedlitz, Epsom, Seltzer, Tunbridge, Spa, Pyrmont, Cheltenham, Scarborough, Vichy, Carlsbad, Hartfell, Harrogate, Moffat, Aix-la-Chapelle, Borset, Barege. A synoptical table, exhibiting, in one view, the chemical compositions of all these different waters, is subjoined at the close of the chapter.

The medical uses of water, are, next, *more particularly* considered. Water in general, as he here states, is useful, in proper quantity, to dilute and suspend the solids which are taken into the stomach, sometimes to stimulate languid digestion; sometimes, perhaps, to retard that which might be otherwise too rapid. In acute diseases, its internal use as a *diluent* is highly beneficial. Tepid water, taken in moderate doses, acts, generally, as an agreeable stimulant to the stomach. A draught of water, at the greatest warmth at which it can be drunk, will commonly give sudden relief from *heart-burn*. To sedentary persons, in acute fevers, for topical inflammations, cold water and the cold bath may be very usefully employed, and in ice for burns. The *warm*, or *tepid* bath, is adapted to prove serviceable in, perhaps, a still greater variety of instances. Artificial chemical waters are not without their use.

Whether medically or chemically considered, this work is, in great part, a compilation. It does not appear that Dr. Saunders has himself actually analysed the different mineral waters of which he here gives the history: but he may have had opportunity to observe the effects of them all, when administered as remedies. His compilation is made with diligence, with discrimination, and with evident care for accuracy. A rich vein of medical good sense runs through the whole work; and it is even much that the authority of a physician so truly respectable as Dr. Saunders, is added to that of the different medical writers, by which the remedial uses of these waters had been before examined and explained.

*General Zoology, or Systematic Natural History.* By GEORGE SHAW, M.D. F.R.S. *With Plates from the first Authorities and most select Specimens.* Vol. I. Mammalia. Kearney, London; 1800. 8vo: 552 Pages, 121 Plates.

AFTER the study of man, what can be more interesting or useful than to examine the different forms, colours, and manners of the lower animals; to trace their local history over the earth, as they are in their respective situations to be met with; to acquaint ourselves with their relations to the uses of human life, as well as with the mutual connections and dependencies of their various species on the rest of nature, and on one another?

We owe to two illustrious foreign naturalists, De Buffon and Linnæus, those systems of natural history by which the knowledge of this science has been, in the course of the present century, the most successfully diffused. De Buffon, writing in a popular language, and with great splendour of style, has become the natural historian of the people: Linnæus, seeking his chief merit in the excellencies of scientific enumeration and arrangement, has become the guide of philosophers. Buffon is quoted as good authority: Linnæus is followed, with implicit reverence, as a master. The school of Buffon is now, almost exclusively, confined to his continuators and editors: all Europe has adopted the arrangement and the terms of Linnæus; and his influence is as great in  
natural



natural history, as is that of Sir Isaac Newton in natural philosophy, or that of Lavoisier in chemistry.

In this volume Dr. Shaw gives the first part of a general system of zoology, in which the arrangement of Linnæus is to be chiefly followed. Quadrupeds, birds, amphibia, fishes, insects, vermes, testaceous animals, zoophytes, are to be comprehended in the system. It is to be completed in ten or twelve volumes. The present volume comprehends the history of the primates, bruta, and feræ, the three first orders in the class of mammalia or viviparous quadrupeds. The peculiar advantages of this system are, as the author hopes, to arise from the combination of the arrangement of Linnæus with the materials contained in the works of Buffon and Pennant; from the correction of errors relative to synonyms; the institution of new species; the addition of more clearly distinctive specific characters; from the communication of those new facts in natural history which have been learned in late voyages to the South Sea, and particularly by observations made in the great island of Australasia, or New Holland. The descriptions are illustrated, as the title-page expresses, with engravings of the forms of many of the animals described. These engravings are copied from the works of Buffon and other naturalists, from various original drawings, and from specimens in the Leverian and the British museum. The history of thirty-eight different genera of quadrupeds is contained in the first volume. The engravings are, in general, well executed by Heath and other artists.

It is extremely probable that the whole work, if conducted to an end with the same care and skill which appear in the first volume, will present a fuller and more correct system than has been as yet published of our knowledge in zoology. Its chief *merits* are, great clearness and accuracy of description, fidelity of representation in the engravings, ease and perspicuity of style, just discrimination as to distinctive characters, and the increase which it affords of the number of known animals. Its chief *imperfections* are, that the author appears to have studied more in cabinets of curiosities, than by conversing with living nature; that he assumes for his author-

rities in facts, rather the writers of former systems, than the narratives of original observations from which those writers compiled; that, while intent chiefly upon accurate description, he has told little, except in a few popular instances, of the manners, habits, and local relations of the quadrupeds which he describes.

*The Asiatic Researches; or Transactions of the Society instituted in Bengal for inquiring into the History and Antiquities, the Arts, Sciences, and Literature of Asia, Vol. VI. Calcutta, 1800.*

We are happy to find that this Society still continues its labours with unabated spirit and industry. The contents of this volume are:

1. A Discourse delivered by Sir Robert Chambers, Knt. President.—2. Narrative of a Journey from Agra to Oujein.—3. An Account of the Inhabitants of the Pogy or Naffau Islands, lying off Sumatra.—4. Observations on the Theory of Walls, wherein some Particulars are investigated which have not been considered by Writers on Fortification.—5. On the Poison of Serpents: Supplement to the foregoing Paper.—6. An Account of Petroleum Wells in the Burmha Country.—7. On the Maximum of Mechanic Powers, and the Effects of Machines when in Motion.—8. On the Religion and Literature of the Burmas.—9. Narrative of a Journey to Sirinagur. Enumeration of Plants noticed in the preceding Tour. Letter from Sir C. W. Malet, Bart. to the President, on the Subject of the following Paper:—10. Description of the Caves or Excavations on the Mountain about a mile to the Eastward of the Town of Ellora.—11. Remarks on some Antiquities on the West and South Coasts of Ceylon: written in the Year 1796.—12. On Mount Caucasus.—13. On the Antiquity of the Surya Siddhanta, and the Formation of Astronomical Cycles therein contained.—Appendix. Rules of the Asiatic Society. Members of the Asiatic Society,



INTELLIGENCE,  
AND  
MISCELLANEOUS ARTICLES.

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LEARNED SOCIETIES.

ASIATIC SOCIETY.

THE Asiatic Society instituted in Bengal for inquiring into the History and Antiquities, the Arts, Sciences, and Literature of Asia, having resolved to give with each subsequent volume of their Researches a list of such oriental subjects as require further illustration, and to invite communications on the subject, have selected the following, and prefixed them to their last volume, lately published at Calcutta.

*I. Religion, Policy, Jurisprudence, Manners, and Customs.*

1. An accurate description of the different festivals and fasts prevalent in India; together with an investigation of their origin, and of the reason and signification of their peculiar ceremonies. As those are very numerous, the following are specified as objects of primary inquiry:

Among the Hindus—Doorga Pooja, or Dufferah; Kalee Pooja, Dewalee, Jonmon Ashtomer, Churkh Pooja.

Account of the pilgrimage to the temple of Jaganatha, at Purfotom.

Among the Musulmans—Eed ul Zoha, Eed ul Fetr, Eed Ghudeer.

2. An enumeration of the different casts of Hindus, with the customs peculiar to each, as existing in the present time.—See an enumeration from the antient Sanscrit records, Asiatic Researches, Vol. V. p. 53.

3. A connected history of the several Musulman tribes existing in India: among these, an account of the singular tribe known by the name of *Borab*, is particularly required.

4. What

4. What kinds of oaths are considered as peculiarly binding by the different tribes and sects in Hindustan?

5. What historical monuments remain of the government, and the system of police which obtained in Hindustan previously to the Musulman invasion?

## II. Geography.

1. A catalogue of the names of towns, countries, provinces, rivers, and mountains, from the Schasters and Puranas, with their modern names annexed; and a correct list, according to the oriental orthography, of the towns, &c. mentioned by Major Rennell and other European geographers. The etymology, as far as practicable, would also be desirable.

2. What were the geographical and political divisions of the country before the Musulman invasion?

## III. Biography.

1. Accurate translations of the accounts given of the life and actions of Bouddha, by the priests of his sect.

2. To inquire if there be any accounts remaining of Chauchasan, Gonagom, and Gaspa; whom the Burmas represent as having preceded Godama.

3. The history of Mahamoony, a disciple or follower of Godama; to whom also adoration is paid by many among the worshippers of Bouddha.

4. A history of those saints, philosophers, &c. either male or female, who have become famous, in modern times, among the nations and religious sects that inhabit India.

## IV. Commerce, Natural History, Materia Medica.

1. To inquire into the state of the commerce of India previously to the first settlement of Europeans.

2. To ascertain the different trees which produce *gamboge*, or a gum-resin resembling it: to investigate the qualities of the drug as procured from each of those trees, among which we may reckon the following:—*Cambogia gutta*, Linn.; *Garcinia celebica*, Linn.; *Stalagmitis cambogioides*, Koen.; *Hypericum pomiferum*, Rox. To procure accurate figures of  
the



the *Stalagmitis cambogioides*, or the Ceylon tree, and of the tree which yields this drug in Cambodia. Lastly, to determine whether all those trees may not be referred to one genus.

3. To ascertain from what country the root commonly called *columbo* is procured, and to give a botanical description and figure of the plant to which it belongs.

4. The botanical names of plants mentioned in the Hindu books of *Materia Medica*.

5. To supply the deficiencies which remain in the accounts of the production of borax in the neighbourhood of Tibet and Napal, as delivered by Mr. Blane and Father Joseph de Ravato in the Philosophical Transactions, Vol. LXXVII.

6. Whether the tobacco plant was known in Asia before the discovery of America; and whether the edict, said to have been published by Aurunzebe, against the use of that plant, be authentic?

#### V. Medicine and Surgery.

1. History of that peculiar inflammation of the schneiderian membrane termed *nakra*, with the mode of treatment by the natives.

2. History of inoculation for the small-pox among the *Hindus*.

3. Antiquity of the venereal disease in India; and the knowledge which the antient Hindu physicians had of its cure.

4. Their treatment of the leprosy; with some account of the different species of that disease which are met with among the natives of India.

5. How long have the natives possessed the art of couching for a cataract, and from what source did they obtain it?

#### VI. Language, Literature.

1. How many dialects are there of the Hinduwee, *i. e.* of languages connected with the Sanscrit; and in what parts of India were they or are they spoken?

2. What general term had the natives of India before the Musulman invasion, to designate what we imply by the term *Hindu*.

3. To obtain as full a catalogue as possible of books in the Sanscrit

Sanfrit and other Hinduwee languages, containing the following particulars, as far as they can be ascertained, *viz.* the names of the authors, the subjects, the dates, the age of the most antient manuscript of each now known to exist; and the places where the books are now to be found.

ELECTORAL ACADEMY OF THE USEFUL SCIENCES  
AT ERFURT.

In the sitting of February the 4th, Dr. G. H. Thilow communicated to the Academy an account of some experiments he had made in regard to Galvanism. These experiments were a continuation of his former ones\*, and tend still further to confirm the idea that saltpetre possesses the peculiar property of weakening the nervous system. This he ascertained by several experiments. Having cut off a portion of the crural nerve, which had been laid bare, and weakened or deprived of its irritability by saltpetre; he laid bare another portion towards the knee, and, by strewing nitre over it, deprived it in a few minutes of all susceptibility of irritation. The same thing always took place as far as the toes whenever a new portion of the nerve was laid bare. The author thence concludes that the vital power is not totally extinguished even when it cannot be excited by the strongest stimulants applied to a nerve which has been several times subjected to the action of some stimulant. The heart of a frog laid bare, and still in connection with the surrounding parts, and which contracted and expanded with great vivacity, being strewed over with a little saltpetre, its movement was by these means immediately checked: though the author had taken the heart from the thorax, the fore-legs of the animal still moved with such strength, that the whole upper part of the body was thereby agitated from one side to the other. Some drops of warm milk, let fall between some of the muscles and nerves, speedily put an end to this violent movement. These striking phenomena the author endeavoured to explain from the circumstances of milk removing obstinate spasmodic affections, and restoring the equilibrium of the muscular and nervous fibres. He had observed also,

\* See *Philosophical Magazine*, Vol. VI. p. 284.



several times, that persons, whose fibres were too rigid and tense, were often troubled with crampish affections, and frequently suffered from violent flatulencies. He always found that their complaints were increased by bitters or carminatives in general, but that they were lessened by the use of lukewarm milk.

The same author read some observations on the use of the German opium. As foreign opium is brought to Europe in an impure and imperfect state, it is of importance to discover some substance that might be substituted in its stead. Professor Trommsdorff found such a substitute in poppy-heads, from which he has prepared a tincture. Dr. Thilow having made some experiments with this tincture in the Clinical Institute of Erfurt, observed that it fully answered expectation in cases of flux, spasms in the stomach, gout, and hysterics\*.

#### FRENCH NATIONAL INSTITUTE.

C. Olivier lately read before this society some observations on the jerboa.

His object was to rectify an error into which the antients and moderns have fallen, in regard to the manner in which the jerboa walks; and to make known the very singular organization of the genital parts of the jerboa; *mus jaculus*, LINN., *mus sagitta*, PALL., to compare them with those of the *alactaga*, and to describe more at length the species mentioned by Linnæus, under the name of *mus longipes*, improperly confounded with the *mus meridianus* of Pallas.

The jerboa is represented in an erect posture on the Cyrenean medals; Herodotus, Aristotle, Theophrastus, Pliny, &c. among the antients, and Paul Lucas, Buffon, Allamand, Pallas, Vic. d'Azyr, &c. among the moderns, have all considered this small quadruped as a biped, that is to

\* It may be gratifying to our readers to learn that a premium for the production of opium in England, offered by the Society for the Encouragement of Arts, Manufactures, and Commerce, was awarded in their last Session to a gentleman who produced a considerable quantity from poppies, which was found, by proper trials, to contain all the requisite properties.

say, an animal which walks on its hind legs alone. Olivier has rectified this error by his own observation, which, on this point, perfectly agrees with the structure of the body of the animal, as it does not allow it to remain long upright on its tarsi.

The penis of this quadruped, as observed by Sonnini \*, is furnished with two long osseous hooks near each other, and placed towards the middle of the upper part of the glans. The latter is guarded besides with *papillæ*, almost of an osseous nature, bent back and shaped like a spoon. In the *alaçtaga*, the *mus jaculans* of Pallas, the glans is merely covered with *papillæ*, in the form of spikes, almost straight, rounded and directed backwards. The testicles are concealed in the abdomen; and the orifice of the vulva in the female appears to be confounded with that of the anus.

The description which Olivier gives of a small species found by him in Egypt, and which, in size, is nearly equal to that of a rat, is perfectly applicable to the *mus longipes* of Linnæus; except that the latter, according to Linnæus, has only four toes on the fore feet, while that of Olivier has five; but it may be possible, says Olivier, that Linnæus did not pay attention to the thumb, which is indeed very short.

As there is great confusion in the synonymy of authors, Olivier endeavours, at the end of his memoir, to rectify their mistakes. At the same time he gives the specific characters of the species, which, in his opinion, belong to that genus.

1. *Dipus cerfer* pedibus posticis tetradactylis.
2. *Dipus jerboa* pedibus posticis tridactylis.
3. *Dipus alaçtaga* pedibus posticis pentadactylis, lateralibus multo brevioribus.
4. *Dipus gerbillus* supra flavus subtus albus; pedibus posticis pentadactylis, digitis subæqualibus.

Decandolle communicated to the First Class of the Institute a monography of the bilocular leguminous plants.

The bilocular leguminous plants are those, the fruit of which are divided into two cells by a longitudinal partition, com-

\* *Voyage en Egypte*, Vol. I. p. 133.



plete or incomplete. Tournefort has given a section of them in his family of the *papilionacei*. Linnæus has established them into three genera: the *bissemla*, characterised by its indented seed; the *phaca*, the seed of which ought to be semi-bilocular; and the *astragalus*, where the seed is bilocular; but these two last genera are not sufficiently distinct; for that reason Lamarck united them. Decandolle has retained that arrangement, but he has changed the characters and rendered them more distinct.

The genus of the *phaca* has for its character a carina, furnished with a long straight point; a seed with two longitudinal cells, sometimes complete, but for the most part incomplete, formed by the folding back of the upper future. This genus comprehends twenty species, viz. *phaca sebirica*; LINN.; *ph. myriophylla*, *muricata*, *sylvatica*, *oxyphylla*, *prostrata*, PALL.; *astragalu verticellaris*, *alpinus montanus*, *campestris*, *pilosus*, LINN.; *astr. foetidus*, VILL.; *astr. deflexus*, PALL.; *astr. annularis*, FORSK.; and seven undescribed species. The *phaca alpina*, *australis bætica* and *rigida*, are referred to the *colutea*. The genus *astragalus* is distinguished from the preceding by its *obtusè* carina, and its seed with two longitudinal cells, sometimes complete, or for the most part incomplete, formed by the *repli* of the *lower* future. It comprehends 125 species, 36 of which are nondescript. This vast genus had been divided into three sections, according as the stalk was herbaceous, ligneous, or null; but these divisions are not correct. Decandolle has divided the genus into two sections, according as the stipulæ are distinct from the petiole, or adherent to it. The first section comprehends the species with yellow or purpurine flowers. The second is divided into three subdivisions; the first has the stipulæ adherent to the petiole and the stalk, and the petiole herbaceous and not spinous; the second has the petiole not spinous and *caduque*; the third has the petiole spinous *persistant*, and the folioles *caducous*. These are the *tragacantha* of which the old botanists made a genus; but the fructification presents no character, and exhibits the same variations as those of the other *astraguli*.

The *phaca*, *astraguli*, *bâguenaudiers*, and several other

genera in different families, have the vesicular pericarpium filled with air. Decandolle has observed, that if this air be analysed at the moment when it is collected from the pericarpium, it is found to be of the same purity as atmospheric air; but if this pericarpium be immersed in water, the air loses its purity, and in the course of about a day no oxygen gas is found in it. The same thing takes place in the sun as well as in obscurity. The total quantity of the air does not appear to the eye to be diminished. Is the oxygen gas converted into carbonic acid gas? or, rather, does it not serve for the nutrition of the seed. What tends to support this idea is, that Humboldt found that the tunics of the seeds contain azot almost in a state of purity. This fact coincides with an observation which Decandolle made before on the vesicles of the *fucus vesiculosus*; he observed that these vesicles contained atmospheric air when they had remained some hours out of water, and azotic gas when they had been some time immersed in it. These facts deserve the attention of physiologists.

It is well known that gum tragacanth is furnished by certain species of the *astragalus*, called on that account *tragacanthæ*. It appears that several species of this division possess the same property; according to Tournefort, the *astragalus creticus* furnishes this gum in the island of Crete; and the *astragalus gummifer* furnishes some of it also in mount Lebanon, according to Lubillardier. It appears that it was the *astragalus echinoides*, from which Prosper Alpinus, as he says, saw this gum distilling. In a word, Olivier assures us that the gum used in commerce comes neither from Crete nor from Lebanon, but that the place of its deport is Aleppo; and that it is brought thither from Persia, where indeed the shrub that produces it is found. It is a *tragacantha* not yet described.

The anti-syphilitic properties of the *astragalus exscampus* have of late been much extolled, and still require confirmation. The rarity of this plant prevents it from being subjected to experiments. Decandolle recommends making them on the *astragalus incanus*, and *monospeffulanus*, which grow in France, and which seems to be analogous to the former.

The



The existence of ornitholites in strata of submarine formation, is still contested by several naturalists. The celebrated Fortis has even published a memoir in which he proves that no well ascertained instances of this circumstance had ever been known. Mention had indeed been made in several works of those found at Montmartu; but some doubts still remained.

C. Cuvier, however, has lately presented to the Institute a fossil which seems to have all the characters of an ornitholite. It is a leg composed of a portion of the femur, a tibia, and a tarsus, in one piece; three toes, one of which has three, the second four, and the other five articulations, and the vestige of a spur. These members are not found but in the class of birds alone.

This leg was incrusted in that kind of gypsum which in large strata occupy such an immense space around Paris. It was found at Ville-Juif, in the third mass, that is to say, about sixteen yards lower than the strata which contain the bones of quadrupeds already described by the same author.

C. Vidron, music master at Paris, having announced a discovery he had made, by means of which persons born deaf could be enabled to hear music. Haüy, Lacepede, and Cuvier, were appointed to examine this discovery, and to make a report on the subject to the Institute. It appears from this report that the apparatus employed by C. Vidron consists of a steel rod, one end of which he places on the lid of a harpsichord, and the other between the teeth of the deaf person. This rod is furnished also with a branch terminated by a knob, which rests in the pit of the stomach, and sometimes also with a second, applied to the cranium. The commissioners mentioned that several authors have announced that certain deaf persons had been made to hear musical sounds by establishing a communication between their teeth and the instrument by means of a rod, goblet, or other body. Of these authors they mentioned Fabricius ab Aquapendente, Schellhammer, Boerhaave, Winkler, and Jorissen.

They endeavoured, in particular, to determine how far this invention might be of utility either in regard to the different kinds of deafness, or in regard to the different kinds of sound,  
which

which one may wish to communicate. They gave themselves an artificial kind of deafness by stopping their ears, and removing to a great distance. In both these cases, sounds were perfectly heard by the steel rod, and the sounds appeared to them to come from the inside of the rod, and not from their real place. But the results were different with respect to some persons actually deaf, on whom the experiments were tried. Some of them heard very distinctly; but the greater number declared that they experienced only a tremulous movement, more or less general.

The commissioners conclude that this method may be employed with success in cases of deafness which arise only from some obstructions in the external part of the auditory passage, but that it is useless in those occasioned by a paralysis of the nerve, or any essential derangement in the interior organs, which, unfortunately, is most commonly the case, especially in those born deaf. They are, however, of opinion, that it ought to be tried on young persons who are deaf; for, even if there were found only one in a hundred capable of deriving benefit from this resource, it would at any rate be one source more of enjoyment to such an individual.

In regard to articulate sounds or words, the commissioners found that it is hardly possible to hope that they can be conveyed with accuracy by this apparatus, at least in its present state.

In the fitting of August 23, General Dugua, who had returned from Egypt, having brought with him two copies of a remarkable inscription found on a black granite of an exceedingly fine grain, presented them to the Institute.

This monument was discovered in digging up the earth at Fort Elleve, near the Bogar of Rosetta, two leagues from that town.

It was not brought to Cairo till after Buonaparte's departure, and was deposited in the Institute of that city. The discovery was made by C. Bouchard, lieutenant of engineers, who was appointed to superintend the repairs of the fort.

Two members of the commission of the arts were employed at Cairo in taking copies of this monument, unique of its kind, in order that it might be preserved whatever might



happen. The first was C. Marcel, director of the national printing-office in Egypt, who possesses extensive knowledge and an indefatigable zeal for acquiring more. By a simple typographic process he obtained copies with the characters inverted, but which could be easily read by means of a mirror. The second was C. Conté, chief of the brigade of aërostats, who, by his knowledge of mechanics and the arts, had always been one of the most useful men in the colony.

He employed the processes of engraving to obtain copies, which at first gave the characters inverted; but these copies being put under the press, gave others similar to the inscriptions.

The inscription is in three different characters; one portion presents a series of hieroglyphics, in several lines exceedingly regular; another which has not yet been sufficiently examined, exhibits a great number of lines in characters which still leave some uncertainty, and which require a more profound research. The last portion is composed of fifty three lines, written in Greek. Laport-Dutheil one of the members of the class of literature and the fine arts, immediately began to transcribe this part; and communicated to the class a part of its contents. He afterwards made known the substance of nearly the whole of it, having transcribed the first thirty-eight lines. According to his explanation, it appears that the inscription is a monument of gratitude from the priests of one or more temples, either of Alexandria or some place in the neighbourhood, to Ptolemy Epiphanes.

A literary man of eminence has already published some observations on the first explanation given of the Greek text. If the inscription be in honour of Ptolemy Dionysius, says he, it follows that the date of it must be 63 years before the Christian æra; on the other hand, if the inscriptions express the same thing, it thence follows that the hieroglyphic language was in use even so late as 200 years before Christ\*,  
though

\* The Apocalypse furnishes a direct proof that it was understood, even subsequent to the Christian æra. If commentators had attended to this fact, and followed the hieroglyphical acceptation of the figures employed

though it is commonly believed among the learned, that in the time of Herodotus, that is nearly 500 years before Christ, the knowledge of this language was already lost.

Since these inscriptions were presented to the Institute, Bonaparte has ordered engravings to be made of them, in order to gratify the curiosity of the learned in all countries.

#### FOSSILS.

Faujas St. Fond has lately brought to Paris some curious fossils, having on them the impression of plants; which he found under lava at the depth of twelve hundred feet. Jussieu, Desfontains, and Lamark, have observed that several of these impressions belong to plants common in Europe at present; such as leaves of the chestnut tree, of the birch, mountain-ash, the maple, an entire cone of the *pinus sylvestris*, and a portion of a cone of the *pinus sativa*. Fabricius and Latreille discovered in them also the impression of an insect, which is the common *hydrophylus* or water-beetle. Faujas St. Fond intends to publish an account of this discovery, illustrated with engravings.

ployed in that book, instead of giving their own opinions, founded on a supposed resemblance of the figures to the objects to which they arbitrarily applied them, they would have come nearer to the author's meaning than they have done.—EDIT.

#### ERRATA IN VOL. VII.

Page 356, l. 21, for *this* r. *their* : p. 360, l. 5. for *her* r. *the* : p. 362, l. 11, for *mere* r. *more* : p. 372, l. 4, for *sulphat*, r. *sulphuret*:



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THE  
PHILOSOPHICAL MAGAZINE.

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NOVEMBER 1800.

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*A short View of the Observations which have been made at different Times on the Luminous Appearance of the Sea. Read in the Physical Society of Gottingen, by J. G. L. BLUMHOFF\*.*

THERE are many things in nature of which we know nothing more than that they exist: respecting the manner in which they exist, and for what purpose, we are entirely ignorant. They are often the objects of our wonder and admiration; but, owing to the imperfection of our faculties, we can acquire no satisfactory knowledge of their nature. The only course, therefore, which, as rational beings, we can pursue in such cases, is to endeavour, by the help of conjectures, to open a way to truth; and, when this cannot be accomplished, to endeavour to approach as near to it as possible. This, indeed, may often be effected with some degree of success, by admitting certain hypotheses, or fundamental principles deduced from analogy, especially when our researches are conducted with accuracy and perseverance. It may be proved by innumerable instances, taken from the history of the sciences, that hypothetical and conjectural explanations have often conducted mankind to the discovery of immutable laws, and truths of the greatest importance.

To explain the phænomena of nature, has always been

\* From *Magazin für den Neuesten Zustand der Naturkunde*, &c. Vol. I. part 4.

the employment of philosophers; and yet there are many of them respecting which our knowledge is very uncertain. But how is it possible for such short-sighted beings as men are, to have at command, and employ in a proper manner, all those external circumstances which are necessary for the thorough examination of such phænomena? A thousand things occur daily in nature which it is impossible for our coarse organs to perceive, and much more to observe in a perfect manner: in this paper, however, I shall not descend to microscopic objects, but offer a few remarks respecting a phænomenon which frequently occurs, and which has attracted the notice of many celebrated philosophers—I mean the luminous appearance of the sea. This phænomenon, which exhibits so magnificent a spectacle to navigators, and which, when beheld for the first time, must excite astonishment, deserves certainly to be examined with more care and attention. I shall here, therefore, take a short view of the information which has been collected on this curious subject.

That the water of the sea sometimes exhibits a luminous appearance in the night-time, especially when struck by the rudder of a ship, or strongly agitated by its resistance to the motion of the vessel, is a circumstance well known; and this phænomenon appears sometimes, in calm weather, like multitudes of small stars dispersed over the surface of the sea. It frequently happens, also, that the places only next the ship or the ship's wake, or the track through which fish have passed, appears luminous. The well-known Americus Vesputius, the Italian navigator, according to Kircher, was the first person who observed this circumstance.

In Lichtenberg's Magazine for the latest Discoveries in Physics and Natural History\*, we find the following description of this phænomenon as observed in the Baltic:—"The phosphoric phænomena in the Baltic generally appear in the darkest nights, with an undulating motion, in the track formed by vessels in their progress through the waves. The water seems to emit a lively light, which is sometimes of a pale red colour, and has the resemblance of sparks. Sometimes it exhibits the appearance of a regular stream of fire in

\* Vol. II. part iv. p. 48.



the ship's wake. This phænomenon is seen sometimes also when the wind blows, but not with that majestic appearance as during a calm. It is worthy of remark, that, when vessels are lying at anchor, this phænomenon is observed here and there, darting itself from one place to another with great vivacity, behind the vessels, even when they make little or no motion." This description agrees in the most essential parts with that of Gentil, who observed the same phænomenon in the Indian seas, particularly in the channel of Mozambique. On one occasion the sea appeared to this navigator and his whole crew to be entirely covered with fire, and each wave to consist of a mass of phosphoric matter. The ship seemed as if moving up and down in a fiery lake. Some of the ropes reflected the light so strongly, that the people on board could have read by it. The fire of Saint Elmo appeared also at the same time on the summit of the mainmast \*.

Boyle† sought for the cause of this phænomenon in certain general laws of the earth, or of our planetary system; but, had this celebrated philosopher considered the subject with a little more attention, he would have found that it may be explained on much better principles.

Father Bourzes‡, during his voyage to India in the year 1704, had several times an opportunity of observing the luminous appearance of the sea, and has given us a very valuable account of the phænomenon. The light was often so strong that he could read by it the title of a book, though he stood at the height of nine or ten feet above the surface of the water. Sometimes he could distinguish the luminous

\* Nous eumes dans ces parages de si mauvais temps, et entr'autres une nuit si épouvantable, que la mer sembloit être tout en feu : chaque lame étoit pour ainsi dire un phosphore : le vaisseau paroïssoit être dans un étang de feu. Nous étions à la cape sous la misaine : cette voile refléchissoit la lumière de la mer à un point, qu'on eût dit qu'elle étoit éclairée par la lumière d'un très grand nombre de fanaux ; et on eût pu lire auprès de la relinque de cette voile, à la faveur de cette lumière refléchiée de la mer. Le feu de St. Elme parut un instant au haut du grand mât.—*Voyage dans les Mers d'Inde*, Vol. I. p. 687.

† Philosophical Transactions, Vol. LIX. p. 450.

‡ Lettres édifiantes, Vol. IX. Philosophical Transactions abridged, Vol. V. p. 213.

parts from those that were not so in the ship's wake. The former exhibited in part luminous points and in part small luminous globules, some of which were from one to two lines in diameter, and others as large as a man's head. Sometimes they formed streaks from three to four lines in length, and one or two lines in breadth: sometimes they resembled luminous vortices, which, according to the author's expression, appeared and disappeared suddenly at certain periods, like flashes of lightning.

Father Bourzes observed also, that the passage of fish through the water was marked by a stream of light, so that it was often possible to distinguish their shape and size. He remarked also a number of luminous particles in the water drawn up from the sea, when he stirred it round with his hand in a dark place. These luminous particles he observed also on a piece of linen which he had dipped in sea-water, when it was wrung in the dark, and even when it was half dry.

This attentive observer considers the principal cause of these phænomena to be a kind of greasy viscous matter in the water of the sea, which arises perhaps from putrefaction, because he thought that the water, the fatter it seemed, and the more covered with scum, always emitted the more light. In confirmation of this opinion he states, that having caught a bonetta, its mouth appeared so luminous in the inside that he could see to read a book by it. On examination, the mouth of the fish was found to be full of a viscous matter, which being rubbed over a piece of wood, the latter became also luminous in the dark, but the light it emitted ceased as soon as the matter became dry.

The experiments of Canton, mentioned in the Philosophical Transactions\*, seem to coincide perfectly with the above description of father Bourzes. I shall therefore give a short account of them, as they are the most accurate we have on the subject. On the evening of the 14th of June 1768, Mr. Canton put some fresh whittings into sea-water, and found that they emitted light for twenty-four hours. In the cellar where he deposited the vessel which contained

\* Vol. LIX. p. 446.



them, Fahrenheit's thermometer stood at  $54^{\circ}$ . The water itself, when at rest, was quite dark; but, when a stick was drawn through it, the whole part through which the stick had passed became luminous, and no light appeared any where else. When he agitated the whole water, it became luminous throughout. When the fish had lain in the water forty-eight hours, it was then brightest: at the end of three days, though ever so much agitated, it emitted no more light.

The sea water, however, exhibited a much more luminous appearance when a herring was placed in it. The third night it was so strong, that, when stirred, a person could tell by it the hour on a watch; and at those moments the fish seemed to be an opaque body. After this period the light decreased more and more, but it still continued, in some degree, till the seventh night.

Another vessel containing fresh water, into which a fresh herring was put in the like manner, continued entirely dark, without emitting the least trace of light. The thermometer during the time stood always above  $60^{\circ}$ .

River water, in which salt was dissolved till it appeared by a hydrometer that it had acquired the gravity of sea water, exhibited the same phænomena as water brought from the sea: in water to which more salt was added, the fish emitted no light.

It appears from these experiments and those of Sir John Pringle\* that the salt in sea water promotes putrefaction; and that therefore the luminous appearance of sea water arises from a disposition to putrefaction, or a commencement of it.

Experiments on the shining of fish in salt water have been made also by other philosophers besides Canton: among these we may mention those of Boyle and Dr. Beal in the Philosophical Transactions†, and those of Martin in the Transactions of the Swedish Academy‡, all of which seem to confirm, in a greater or less degree, that the luminous appear-

\* Experiments on Septic and Antiseptic Substances.

† Philosophical Transactions, No. 31, p. 581; and No. 13, p. 226.

‡ Swedish Transactions, Vol. XIII. p. 225.

ance of sea water is owing to a commencement of putrefaction\*.

Another circumstance which has been proved by these observations is, that heat deprives putrid bodies of their luminous property. This circumstance did not escape Canton; for he remarked that, though a strong summer heat promoted putrefaction, a heat twenty degrees higher than that of the blood seemed to prevent it. This he discovered by the following experiment:—He put a small piece of a fish which had become luminous, into a globe of thin glass, immersed it in water heated to  $118^{\circ}$  of Fahrenheit, and observed that it was deprived of its luminous property in less than a minute: on being taken from the water, however, it recovered its luminous property in ten seconds, but the light it emitted was not so strong as before.

Spallanzani does not agree in opinion with Canton, that the luminous appearance of fish arises from putrefaction†. He repeated Canton's experiments with a variety of fish, but he succeeded only with a very few of them. Those which did not emit light he found in general to be the fattest, though, according to Canton, these ought, above all others, to communicate a luminous appearance to sea water. Another circumstance, which seems contrary to Canton's hypothesis, is, that if the nocturnal luminous appearance of the sea were occasioned by the oleaginous remains of putrid fish, as these remains always float at the surface the light ought to be confined to the surface of the water; which experience shows not to be the case.

Vianelli, a physician of Chioggia in the neighbourhood of Venice‡, Grizzelini, a Venetian physician, the abbé Nollet§, and Dr. Priestley, ascribe the luminous appearance

\* That several kinds of fish, such, for example, as the *sepia*, shine in the dark, is well known.

† *Memorie di Matematica e Fisica della Soc. Italiana*, Vol. II. p. 603.

‡ *Nuove Scoperte intorno le Luci notturne dell' Aqua Marina.*—*Venez.*

§ *Nouvelles Observat. sur la Scolopendre Marine*, and the abbé Nollet, in the *Memoires de l'Acad. des Sciences* 1750, p. 88.



of the sea to phosphoric insects\*. The abbé Nollet, in particular, while at Venice in the year 1749, paid great attention to this phænomenon, and was confirmed in his opinion by the following observation:—Having remarked that the sea water in the harbour of Portofino emitted a strong light, he lay down on the edge of the water; and, stretching his head over it, he saw these insects spring up, as insects in general do, from the bottom of the water, which was covered with sea-weeds. When he attempted to catch them in his handkerchief, he found on it only luminous spots, which he could spread over it with his finger.

Le Roi, during a voyage up the Mediterranean, observed that the head of the ship, as it passed through the waves, threw up in the daytime a multitude of small particles, which at night had a fiery appearance†. He is not, however, inclined to ascribe this phænomenon to insects, because, having collected some of these luminous particles in his handkerchief, he found them to be round bodies like pin-heads, without any appearance of animal organs, though he examined them with a microscope. But he does not deny that there are luminous insects in the sea, though he is of opinion that its luminous appearance arises from some other cause, which he does not further explain.

Fougeroux de Bondaroy‡ and father Torrubia§ ascribe the cause of the luminous appearance of the sea to a multitude of small nereids, the hinder parts of which the former found to be endowed with the property of emitting light. This is the case in particular with the *nereis noctiluca* Linn. ||

\* Some of the scolopendræ, as the *scolop. electrica* and the *scal. phosphorea* L. really shine in the night-time. According to Hablitzl (see *Goth. Mag.* Vol. II. Part 4, No 159), a cable drawn up from the sea emitted a very lively light, which arose from its being covered with the sea flea (*cancer pulex*); a proof that those animals shine in the dark.

† Observat. sur une Lumière produite dans la Mer, in the *Memoires* présentées, Vol. III. p. 144.

‡ Sur la Lumière que donne l'Eau de la Mer principalement dans les Lagunes de Venise, in the *Mem. de l'Acad. des Sciences. Paris* 1767, p. 120.

§ Vorbereitung zur Naturgeschichte von Spanien. *Halle* 1773. 4to.

|| See Linn. *Syst. Nat.* Vol. I. edition 13. p. 1085; also his *Amœnitat. Acad. Voi. III. p. 203. tab. 3.*

The celebrated Forskäl, who accompanied Niebuhr as naturalist in his travels through Arabia, having caught a great number of marine animals, among which there were different kinds of *medusæ*, put some of them into a bucket full of water. After keeping them some time, he threw the contents of the bucket from a window, in the dark, and observed that all those places where the water fell seemed to be covered with sparks of fire. He made several experiments on this subject, all of which convinced him that the luminous appearance of the sea arises from insects. Bartholinus also, and Donati, ascribe the luminous appearance of the sea to marine worms (*moluscæ*).

Silberschlag ascribes the luminous appearance of sea water to phosphorus, the component parts of which may exist in the sea as well as in the atmosphere. It is not improbable that several kinds of worms, which, according to various observations, have been considered as luminous, may be indebted for that property to marine phosphorus adhering to them. It has been found, by repeated observations, that the water of the sea, even at the depth of forty feet, is phosphorescent; and the abbé Spallanzani is therefore inclined to believe that this is the case also at every depth. Respecting the luminous appearance of the sea water, however, he gives no decisive opinion, and considers every thing hitherto said on the subject as mere conjectures.

Mr. Erich Schytte remarks, that when sea water is converted into ice it retains its luminous property; and that, when sea water is distilled, the luminous matter does not pass over, but remains behind in the still.

Professor J. Mayer, in a paper published in the Transactions of the Royal Bohemian Society on the luminous appearance of the Adriatic, says, that the water at the surface seems to imbibe the rays of light in the same manner as a light magnet. This luminous appearance is observed more at the surface than at any depth in the water, and disappears when, by violent agitation of the waves, the water at the surface becomes mixed with that below. This author, however, is of opinion, that there are in the water foreign particles which emit a stronger light, and which may be separated

from



from it, but there are no proofs that electricity has any share in the phænomenon.

M. Bajon, on the other hand, considers the luminous appearance of the sea as electric, and, in support of this opinion, remarks that it is never observed but when friction is produced by some body passing over or through the water. For this reason the light is always seen very strong around ships, and where great numbers of fish are collected together. The action of the atmosphere may also excite this light at the surface of the sea. It is promoted by a north wind, and is interrupted by a south wind, as well as by damp weather, &c. This opinion, in the most essential parts, agrees perfectly with that of M. De la Perrier, and of J. R. Forster.

M. Le Gentil also concludes, from observations made in the Indian seas, that this phænomenon is produced by the electricity of the sea water; for he observed it only under certain cases; as for example, when the heavens were filled with clouds, and the sea high, and much agitated. He observed it, however, during calms; but this was never the case except when a storm was to be apprehended. As long as the storm continued, the sea had a luminous appearance; but as soon as it was over, the light ceased. When the weather was in the usual state, that is to say, when the winds were regular, even though they were strong and the sea rough, he observed nothing of the phænomenon. He sailed upwards of six hundred leagues with a strong west wind and very high sea, but did not observe the least spark of light. They appeared most abundant in winter, which is the period when the winds change, and when storms and hurricanes take place. M. Le Gentil asserts that at this period of the year he never wished to observe the sea luminous, because he considered it as a sign of bad weather, and could always, with certainty, foretel by it when a change was to take place. This phænomenon is more common in the Indian seas than in those parts on this side of the Cape, between America, Europe, and Africa. M. Le Gentil ascribes the electricity of the sea to the dashing of the waves against each other, which must naturally produce friction.

I hope I shall be forgiven if I here add the following va-

luable observations on this subject by professor Forster, of Halle. This celebrated navigator and naturalist observed this phænomenon on the night of the 29th of October 1772, during a fresh gale, a few miles from the Cape of Good Hope. He saw also a great quantity of small luminous bodies floating on the sea, some of them near and others at a considerable distance from the ship. He caused some of the sea water to be hauled up in a bucket, in which he found an immense number of small globules which moved with great rapidity. When the water was suffered to stand some time at rest, these luminous globules became less numerous; but, when agitated, they resumed their luminous property. Mr. Forster found, when he examined some of these globules, that they had life and organisation, but they died before he could wash them from his fingers.

Professor Forster distinguishes three kinds of light in the sea, which seems to agree pretty well with the beforementioned observations. The first kind, he says, must be ascribed to electricity, because the quick motion of ships through the water, especially when the wind is strong, occasions a violent friction; and because the agitation of the waves produced by the wind heats the waves more than the atmosphere above them. The resin, pitch, and tar, with which the outside of ships is covered, and the conducting property of water, render the probability of this phænomenon being connected with electricity still stronger.

The second kind of light appears, in a proper sense, to be phosphoric. A great many animal bodies, by putrefying in the sea, become decomposed, and consequently their component parts, and particularly the phosphoric acid, are disengaged. An addition of inflammable matter forms with this acid that mixture which is commonly known by the name of *phosphorus* \*. Thus, fish which are dried in the air become sometimes phosphoric; and this is the case with the ocean itself, when, after a long calm, it has been filled with

\* Professor Forster's reasoning, here quoted, is agreeable to the old theory. We have no right to alter his words; and it will be seen by our chemical readers that the facts might as easily be accommodated to the modern theory.



putridity and corruption; as heat and tranquillity contribute to promote the speedier solution of animal substances. Fish, as well as gelatinous animals, contain oily and inflammable particles, with which the liberated phosphoric acid may readily unite and form a phosphorus on the surface of the sea, and give rise to this wonderful phænomenon.

The third kind of light arises, no doubt, from living animals which float in the sea, and must be produced by their peculiar organisation, or rather their component parts, which deserve to be better examined by chemical experiments.

These conjectures of Dr. Forster contain, in my opinion, the most satisfactory explanation of this phænomenon. All the effects of it hitherto observed may be referred to some of the above-mentioned causes; and the general conclusion that may be deduced from them is, that as these effects have been seen under different circumstances, they must be explained by different causes.

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## II. *On the Submersion of Swallows in Autumn.*

**I**N our fourth Volume, p. 414, we laid before our readers an interesting letter on this subject from Mr. Peter Cole to Dr. Mitchill of New York, to which we refer. Such relations are often questioned; but in natural history, as in other branches of science, the evidence of positive facts is never to be controverted by mere inferences from other supposed or real phænomena. Mr. Cole is a gentleman whose veracity will not be questioned by those who know him, and we have therefore no right to doubt the truth of former relations of a similar nature merely because we know not *now* the character of the relaters. The late Hon. Daines Barrington took the trouble to collect a number of testimonies of similar submersions observed in England, which were published in the Philosophical Transactions, and we subjoin another testimony of such phænomena being observed in the New World.

“ New York, 18th July 1800.

“ On the afternoon of the 24th of August 1798, I was sitting in my parlour, which looks towards the North River,

River\*, about fifty feet from the bank, in company with our mutual friend Mr. Jacob Sebor. Our attention was attracted by numerous flights of birds, which appeared to come across the town from the eastward, and descend immediately into the river. So singular an appearance excited our particular observation. We went out and stood close to the bank, and then perceived that what we at first imagined to be black birds were actually swallows; and that, as soon as the various flocks had cleared the houses, and got directly over the river, they plunged into the water and disappeared. This was not confined to the vicinity of the place where we stood, but was the case as far as the eye could reach, up and down the river, and continued without cessation for nearly two hours, when the closing of the evening prevented our further observation.

“ Aware of the importance of affording any additional information on this long disputed question in the natural history of the swallow, I procured a telescope, and watched attentively many of the flocks from their first appearance until their immersion, continuing my eye fixed upon the spot long enough to be fully convinced that not one of the birds returned to the surface again. Indeed, one flock of about two hundred birds plunged into the water within thirty yards of us, and instantly disappeared, without the least appearance of opposition that might be expected to arise from their natural buoyancy; and at the same time the evening was so serene, and the river so unruffled, that no deception of our sight could possibly have occurred.

“ When the birds first came in view, after crossing the town, their flight was easy and natural; but when they descended near to the water they appeared much agitated and distressed, flying in a confused manner against each other, as

\* The house of Mr. Pollock is situated near the margin of the Hudson, about two hundred yards from the battery. The river is about a mile and a half wide, and from seven to nine fathoms deep, and runs with a strong and rapid tide. Mr. Pollock does not recollect the species of swallow which then disappeared. The barn swallow (*hirundo rufſica*), chimney swallow (*hirundo pelagiæ*), the sand or bank martin (*hirundo riparia*), and the purple martin (*hirundo purpurea*), all frequent and build their habitations in this city and its neighbourhood.



if the love of life, common to all animals, impelled them to revolt against this law of nature imposed upon their species. 'As some time has elapsed since the above-mentioned facts occurred, I thought it proper, before I gave you Mr. Sebor's name, as having been a witness to them, to consult his recollection on the subject, and I have pleasure in assuring you he distinctly remembers every circumstance I have recited, and of which I made a memorandum at the time.' It may be worthy of remark, that, as far as my observation went, the swallows totally disappeared on the 24th of August 1798; for, during the remainder of that year, I did not see one.

"H. POLLOCK."

III. *Account of a fatal Accident which happened to a Traveller on the Glacier of Buet; with some Cautions to those who through Curiosity may visit the Mountains of Switzerland, and particularly the Glaciers. By M. A. PICTET, Professor of Philosophy.*

[Concluded from Page 61.]

C. D'EYMAR, in consequence of a report from two officers of health, respecting the inconveniences which might arise if Eschen's funeral should be deferred, gave the necessary orders in our presence for interring the body in a proper and decent manner. For this purpose we made choice of a spot which would necessarily be seen by every traveller going to Chamouni, and even by those who might ascend the glacier of Buet by the route of Servoz and Villy, in consequence of the intention expressed by the præfect that a monument erected over the grave of this unfortunate young man, with a suitable inscription, while it preserved a remembrance of the event, might forewarn travellers of the dangers to which they would be exposed in traversing the glaciers without taking proper precautions, and without paying to the information of their guides that attention to which it ought to be entitled. While preparations were making for the interment, we interrogated C. Deville and his companions with much interest,

interest, respecting their expedition to the glaciers, and the following account is drawn up from minutes written down at the time :

“ In consequence of the order of C. d'Eymar, præfect of Leman, C. Joseph Maria Deville, after causing a harpoon to be forged, and providing ropes, set out, accompanied by his two sons John Claude and Bernard, and Joseph Ettle, inn-keeper at Servoz, in order to proceed to the glacier of Buet in search of the stranger swallowed up by a fissure in the snow. They left Servoz at seven in the evening, and travelled the whole night.

“ Having arrived on the glacier at day-break, they immediately repaired to a hut built of slate in the neighbourhood, called Castle Pictet, near which, as they were informed, the accident had taken place. They observed some signs of a fissure covered by the snow, but there was no aperture. They however continued their search, and about two in the afternoon Deville found in the snow a hole almost square, about two feet in extent each way, but of which they could not distinguish the bottom. In the neighbourhood there appeared some signs of the existence of a fissure beneath the snow.

“ By means of a stone tied to the end of a rope in the form of a sounding-line, they were able to discover in this hole, at the depth of more than a hundred feet, the presence of a body of a nature different from snow or ice. They then let down the harpoon, which seemed to have laid hold of something, but which brought up only a single hair. Bernard the son then proposed to descend attached to a rope, which he accordingly did ; and when he arrived at a depth where there were only eight inches distance between the sides of the fissure, he was able to touch, with a staff five feet in length, the head of the dead body below him. He then caused himself to be hoisted up, because he found his position exceedingly confined, without having power to move any of his limbs.

“ The harpoon was again employed, but it brought up nothing except fragments of the clothes and a hat. Night came



came on, the weather was cold, and the travellers were wet. They were obliged, therefore, to return to the Chalets de Villy, where they arrived about ten at night.

“ Having consulted together, they resolved to return to the glacier, after supplying themselves with some pieces of wood and some more ropes. They set out from Villy at break of day, and on reaching the mouth of the fissure they constructed a kind of apparatus, by means of which Deville the father was let down; but he was stopped by the narrowness of the fissure, the same obstacle which had stopped his son. He had carried down with him a short-handled hatchet, by means of which he endeavoured to enlarge the passage towards the place where the body lay, in order that it might not be covered by the fragments of the ice. He descended gradually towards it, and endeavoured, but in vain, to move it, as it was so strongly wedged in between the sides of the pit. He then endeavoured to cut the ice around it, and at last disengaged the upper part of the body in such a manner as to be able to fasten a rope round it below the arms. The body was in an erect position, the arms raised up to the head, and the face turned towards the left shoulder. It was quite frozen.

“ Deville then called out to the assistants to pull the rope, in order to try whether it was possible to raise the body, but the attempt was fruitless. He then continued to clear away the ice around, which he at length accomplished, after three hours labour with his hatchet. When this was completed, he first caused himself to be drawn up, and then the body; by this time it was five in the evening.

“ Search was then made in the pockets of the deceased, in order to find out and collect all his property, and an inventory of them was subjoined to the present minutes. A sledge was made with some pieces of wood, and in this manner the body was conveyed beyond the glacier. The brothers Deville then carried it on their shoulders, in turns, as far as the Chalets de Villy, where they arrived at ten in the evening. It was then placed on a mule, and, after resting an hour, they again set out, and arrived at Servoz between five and six in the morning. Bernard, Deville's youngest

youngest son, being worn out with fatigue, was obliged to remain behind at the Chalets de Villy."

Deville himself was entirely exhausted; and, indeed, those who reflect on the labour he had to endure for several hours in the confined place in which he was suspended, and the length of the way he had walked, will be astonished that he could hold out so long. He refused to take any repose until he had assisted, along with us and a very numerous company, at the funeral obsequies of the unfortunate traveller. He was specially charged by the præfect to cause a temporary pyramid to be raised in order to mark the spot.

The compression which the unfortunate young man had experienced by an accelerated fall of more than a hundred feet, in a fissure the sides of which approached each other in the form of a wedge, had been so violent that his watch was beaten flat. We found in his pockets 78 livres in money, and the third volume of Saussure's Journey through the Alps. Such of his effects as were of any value were given to Deville, according to the agreement made by M. Zimpffen, and the præfect engaged to transmit the rest to his relations. We thought proper to add to them a lock of the hair of this interesting youth.

In his pocket-book was found a letter which he had begun, written in German, and directed to his father. We cannot resist the temptation of publishing some fragments of it; they display a just spirit of observation, and great sensibility of mind: I hope his unfortunate parents will forgive us! It is the part which we take in their grief that renders us perhaps indiscrete in publishing the letter of the son; but we wish to make our readers participate in their regret, and to announce, wherever this work may circulate, what kind of son these parents have lost; what hopes have been snatched from them for ever\*.

We

\* The following is a literal translation of this letter, dated Vevey, August 2, 1800:—"You see, my dear father, by the date of my letter, that I have undertaken a journey: you see also that this journey is one of the most interesting that can be wished.

"I set out on Tuesday from Rumlingen and on Wednesday from Berne, and I shall not return to my present habitation before a fortnight

or



We left Servoz a few minutes after the dismal ceremony, and arrived about midnight at Geneva, having our minds and hearts filled with the events of our journey.

One of the dangers to which travellers are exposed in these mountains must be already known to my readers; but this is not the only one, and a simple enumeration of the accidents which have taken place in the course of a few years back, will exhibit others which may sometimes occur.

One of our countrymen, young and active, having ventured without a guide, and without proper shoes, to traverse the steep and rocky declivities which form the base of the needle of Charmoz towards the glacier called *Des Bois*, was precipitated into one of these ravines, and perished.

A young inhabitant of Zurich having ascended to the

or three weeks, when I shall embrace my dear and beloved pupils, my Rudy and my Sophia. It is needless to tell you that I travel on foot, and you may also guess that I travel in company with a friend; for the heart and mind never enjoy the most beautiful aspects of nature but when conscious that a living being sympathises with us. Every thing beautiful and sublime, in order that it may make a deep impression on the soul of man, ought to be united with the enjoyment of love and friendship....

"Scarcely have you reached the canton of Fribourg when you lose sight of that opulence, civilisation, culture, and those natural beauties, which distinguish the canton of Berne; and the contrast is painful. I have found here a confirmation of the opinion I long before entertained of the old government of Berne. However deficient it might be in many respects, it was incontestably superior to all the other governments of Switzerland.

"In the hermitage (near Fribourg) there is now living an old hermit with a long white beard called *Le Frere des Bois*, a name given here to hermits. Though pretty far advanced in years, he is still very fresh. He served a long time as an officer in a Swiss regiment in the pay of Austria. We were very much surprised, when we ascended towards him, to see him coming slowly to meet us across the large hall; not in the dress of a hermit, but in a hussar uniform; his short open red cloak, bordered with fur, and his white breeches, formed a contrast with his long snowy beard. He received us with great kindness, spoke to us in French and German, complained a great deal of the depravity of the present age, and told us that it was not sufficient to pray, it was necessary to act, &c. He congratulated himself on the tranquillity of his life, his habitation being at a distance from all bustle, and his ears being assailed by no noise, except that occasioned by the fall of a neighbouring torrent, and the singing of birds."

summit of an insulated rock which terminates the mountain of Baime to the north, and at a little distance from the neck or passage of that name which conducts from Chamouni to the Valais, becoming giddy, made a false step, and was dashed to pieces at the bottom of the precipice.

A Genevese family, who had gone to visit the ice cave of Arveron, and who happened to be there at a time when the arch fell down in successive fragments, in consequence, it is said, of a pistol being fired, having been so imprudent as not to retire in proper time, while the accumulated ice obstructed the torrent, one of the sons was crushed to death by the falling blocks of ice, and hurried away by the waves; another was severely wounded; and the father, after both his legs were broken, escaped from destruction only in a manner almost miraculous.

These, however, are the only unlucky events which have taken place for a long time in a country visited by many of the curious, who are sometimes imprudent. It may readily be believed that none of these accidents would have happened, had those who fell victims to them listened to the voice of common prudence, which would dictate nearly as follows:

That there is very little merit or glory in exposing one's life, through ostentation, in order to display a courage in which the most common rope-dancer will always be superior to the traveller who attempts to give proofs of his good head or agility in attempts more or less dangerous.

That no one ought to think of traversing these mountains except when conducted by a robust, prudent, and experienced guide. Nothing can be more treacherous and deceitful than those things which appear easy when seen at a distance; those apparent passages, in which travellers are gradually involved, without reflecting that, if they are at last stopped by fear, or by the impossibility of proceeding further, this fear will double the difficulty of retreating, because it destroys that coolness which is necessary for extricating one from disagreeable situations. I have more than once experienced that it is possible to recover this coolness by resolving to look only forwards, and endeavouring, by an effort of the imagination,



to suppose that you are walking merely on the highway, or stepping from one stone to another, or along the top of a stone fence in order to avoid wetting your feet.

In a word, it is necessary to place the most implicit confidence in the cautions of your guide. Men of that class, when they accompany a traveller, lie under the weight of great responsibility. If an accident should take place through their fault, it would ruin their reputation; and on this reputation their maintenance depends. The guides, therefore, are interested by one of those motives which are dearest to man, to give good advice; and the traveller ought to obey it. From these precautions, which may be called moral, I shall now proceed to the physical.

The most essential of all is proper shoes. Three kinds of difficult declivities are found on the Alps—rocks, declivities of ice, and those covered with grass, which are more slippery than ice when the sole of the shoe is smooth, as it generally becomes by walking over them. The use of cramp-irons either on the heel, as Saussure recommends, or placed across the sole, as they are used by some of the mountaineers, will render travelling over these declivities secure to a certain degree: but they must often be put on and taken off, because they render walking more or less cumbersome; which is a great inconvenience. I have found so much utility in an invention which I substitute for them, that I do not hesitate to recommend it to those fond of visiting these romantic scenes.

The traveller must provide strong shoes, the soles of which are at least six lines in thickness, and having the upper leather and quarter double to a certain height around the sole. The upper leather must be exceedingly pliable, in order that it may nowhere hurt the foot, and the shoes should be tried several times in short excursions before a long journey is undertaken with them. Nails of tempered steel must be provided, the tails of which form a screw; their heads, not less than four lines and a half in diameter, must be cut into the form of a square pyramid, which will have two points in consequence of the notch cut into each for receiving the screw-driver to fix them in the shoes. Twelve of these nails

must be put into each sole, *viz.* seven around the fore-part, disposed at equal distances, and five around the heel, as near the edge of the sole as possible without endangering the bursting of the leather. The interval between these nails ought to be filled up with common large-headed nails, so close that their heads may touch each other.

Shoes of this kind will ensure safety to the traveller in all places of difficulty; the nails will hold on granite as well as on the grass, will occasion no inconvenience in the plains, and will last for a long time. When the steel heads become worn down, others may be substituted in their place. The first shoes of this kind, which I caused to be made a dozen years ago, and which I have frequently employed during that period, are still fit for service.

A staff, five or six feet in length, shod with iron, is also useful to those who traverse the glaciers, either for sounding the deceitful snow which covers the fissures, or for walking with more safety over the slippery ice. Staves of this kind, ready prepared by the guides, may be found at Chamouni.

When the traveller has to ascend for a considerable time, it is a bad plan to be desirous of hurrying. By these means the traveller becomes heated, and loses breath, and his progress is more retarded than he expected. The guide must be ordered to walk foremost; and it will be necessary to be directed by his steps, which are measured, as it were, in cadence, and to stop from time to time, but without sitting down, and without halting too long to catch cold. He may ascend in this manner, without stopping, 200 fathoms perpendicular height, and that is enough. He must pull off his coat when he begins to perspire, and carry it folded up on his shoulder; in this manner the heat produced by the labour of walking may be moderated, and he will find, when he halts, the benefit of an additional covering, without any of its inconveniences. In regard to dress, it is of essential advantage, when a traveller sets out for the glaciers, whatever may be the heat in the plain country below, to take proper precautions to guard against the cold, which it is not always possible to foresee. To the ladies in particular I recommend this attention.

These



These precepts are addressed, more or less directly, to all travellers who may be desirous of traversing the mountains; but I shall add a few words to those who are philosophers or naturalists, and to the amateurs of lithology in particular. The latter have frequently experienced the inconvenience of having nothing but their pockets to contain the specimens of rock which they detach with a hammer: these are soon filled, and harass the traveller by their swinging movement. I supply this deficiency, with advantage, in the following manner:

To a pretty broad leather belt I have adapted a loop of the same material, placed in an inclined position, to receive my hammer, nearly in the same manner as the Turks carry their poniards. On the other side is a small pocket, which contains a bottle of acid in a wooden case, a piece of steel for striking fire, &c. This belt or girdle forms the upper part of an apron of very thin leather, which, when let down, would cover the knee, but which, when tucked up on each side, forms a large horizontal pocket before, open at the top, and supported in the middle by a thong shaped like an inverted Y; the two branches of which are sewed to the girdle, and pass round the apron below. The tail of the Y is turned up before, and is buckled to the bandolier with which I carry my barometer. The stones which I put into this pocket being disposed, as it were, around the centre of gravity of the body, and being supported in part by the shoulders, on account of the thong which envelops the apron, occasion no inconvenience. I thus have them all under my eye, and at hand when I wish to substitute one specimen for another; and they experience no agitation, and do not rub against each other as they do in the pockets.

To the same girdle, on one side, are suspended, by steel hooks, one of Ramsden's sextants of three inches radius, which gives even the minutes of a degree; an instrument exceedingly convenient for measuring angles. On the other side is suspended an artificial horizon, with a spirit level for taking heights. The box of this instrument I dispose in such a manner that it serves me for a stand when necessary, being supported by a staff that opens at bottom with three  
feet;

feet; it serves also as a support for my barometer, and makes an excellent walking-stick, when the three branches are folded together. Though the whole of these instruments, added to the usual quantity of stones collected during an excursion of this kind, amount in weight to more than twenty pounds, I am not much incommoded by the burden, on account of its being equally distributed; and in my last Alpine journey, which I undertook with the unfortunate Dolomieu, he envied my lot, though, instead of being accounted as I was, he carried only his hammer and his specimens.

IV. *A cheap and efficacious Method for destroying Rats and Mice; recommended to the Agriculture Society of Manchester\* by Mr. C. TAYLOR, Secretary to the Society for the Encouragement of Arts, Manufactures, and Commerce.*

**I**N or near the places frequented by these vermin, place upon a slate or tile one or two meat spoonfuls of dry oatmeal; lay it thin, and press it flat, that you may more easily know what is taken away. The rats, if not interrupted, will come regularly to feed there; supply them thus with fresh oatmeal for two or three days; then to about six spoonfuls of dry oatmeal add three drops of oil of aniseeds; and having stirred the mixture well together, feed them with this for two or three days more; then for one day give them only half the quantity they have usually eaten of this scented oatmeal, and on the following day place the following mixture:

To four ounces of dry oatmeal, scented with six drops of oil of aniseeds, add half an ounce of carbonat of barytes, previously pounded very fine in a mortar, and sifted through a little fine muslin or cambric: mix this intimately with the scented oatmeal, and lay this mixture of oatmeal and barytes upon the tile or slate, as the oatmeal had been usually placed, and allow the rats to come and eat of it for twenty-four hours without interruption.

\* For this communication Mr. Taylor received the thanks of the Society. As that Society has not yet published any of its transactions, our inserting this article cannot fail to prove highly useful.

A few



A few hours after eating thereof, you will frequently see some of them running about as if drunk, or paralytic, but eventually they generally all retire to their haunts and die. As rats are extremely sagacious, it may be proper, where they have only eaten a small portion, to allow the mixture to remain for forty-eight hours. It will be best to burn what is left after that time, as a fresh mixture may be prepared at a trifling expense, when wanted.

During the time in which the mixture of barytes is exposed to the rats, it is necessary to keep shut the doors of the places where it is laid, to prevent the vermin from being disturbed, or a possibility of accident to any other animal or person; for though it is not so extremely dangerous, if taken internally, as the preparations commonly employed for killing rats, and is even in some cases used in medicine, yet it is fatal if taken improperly.

The oil of aniseeds renders the mixture disagreeable to dogs and many other animals, but is, in small quantities, alluring to rats.

The carbonat of barytes may be procured in large quantities at the lead mines belonging to Sir Frank Standish, Bart. at Anglezark, near Chorley: the proper sort is tasteless, semi-transparent, and effervesces with acids: it is moderately hard and striated. It is frequently called aerated barytes (*terra ponderosa aërata*), and sometimes, by the miners, ponderous spar.

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V. *A brief Examination of the received Doctrines respecting Heat or Caloric.* By ALEXANDER TILLOCH. Read before the Askesian Society, December 1799.

[Continued from Page 78.]

A QUANTITY of heat added to that already in water of a known temperature, which shall *sensibly increase* the volume of the compound, heat and water, only *one thousandth* part, is *sensible* heat; caloric not in combination, not in chemical union—it is cognisable by our senses! But if enough be added *sensibly to increase the volume to one thousand times*  
its

*its original bulk*, then the added heat is latent; it is in chemical union; it is combined caloric! Because now not cognisable by our senses? No. We see the volume of the steam much larger than that of the water; and the more heat we add, the greater is the increase of volume while water is left to receive it. Yet I am not to believe that the heat is cognisable by any organ of sense or external sign! In other words, I am commanded to believe as true, a statement which the evidence of my own senses makes it impossible I can give credit to.

But, say the advocates for this doctrine, a substance so charged with caloric as to become vapour, will not indicate, by the thermometer, any further increase of temperature, though we continue to pour heat into the liquid that produces the vapour; therefore the extra heat must have become *latent* in the steam that has been generated. Nay, we can prove the fact; for this same latent heat may again be made *sensible* in the common process of condensation.

This mode of reasoning appears to me to be more specious than just. It is demanding that the common thermometer should do in this case what it never does in any. This instrument never tells the quantity of heat passing into any body, even in those cases where heat is counted *sensible* or *free*; it only tells the comparative quantity passing into itself from the body in which it is immersed, or with which it is in contact, to bring it into equilibrium with that body as to heat.

Boiling water, steam, the materials of which the thermometer is made, become each charged with heat *in proportion to their capacities*, and this whether the thermometer be in the water or in the steam. The thermometer kept in the steam will never rise higher than  $212^{\circ}$ , “because there the heat is *latent*!” Keep it in boiling water for a year, and it will not rise higher than  $212^{\circ}$ ; yet there the heat is *sensible*! Is this distinction reconcilable with common sense? But the result may even be altered at pleasure. It will not rise higher than  $212^{\circ}$  in boiling water *under the common atmosphere*:—in other words, if we wish to raise the heat higher, we must put a greater pressure upon the water. Confine the water so  
8
that



that none of it may escape, and the heat will rise in it far above  $212^{\circ}$ . Confine the steam, and in it the heat will rise just as high as in the confined water; yet in the one the heat exists in a different state from what it does in the other!!—The result, we see, may be altered by mechanical contrivances: nay, strictly speaking, What is the effect produced by the atmosphere but mere mechanical pressure? Yet we are to believe that a change has been effected in the *physical properties* of one of the substances subjected to its *mechanical operation*!—If Nature had so constituted the atmosphere as to have only half its present gravity, the point at which heat would become latent, as it is called, in steam would have been far below  $212^{\circ}$ . When water is made to boil in an exhausted receiver at a lower temperature, have we done any thing but removed weight from its surface, and *vice versa*?—Does the heat in the steam in these cases pass into a latent state also? If it does, the effect is mechanical: if it does not, then the mere accident of the atmosphere being of its present weight, has nothing to do with the boiling point happening to fall at  $212^{\circ}$ ! But he that would say so would be counted mad.

At Munich, and other places equally elevated above the level of the sea, that is, having a less weight of atmosphere upon them, water, in open vessels, boils at  $209^{\circ}$ . In a partially exhausted receiver the same effect takes place: and yet the doctrine of latent heat is never considered as inconsistent with the fact; really for no other reason but because a common thermometer cannot measure *specific* heat.

To demand that a thermometer should measure the quantity of heat poured into water to convert it into vapour, and to maintain it in that form; and to insist, because the instrument will not do this, that the heat must have changed its nature, and lost its original character; is about as wise as it would be to demand, that a pint measure dipt into the ocean should determine the quantity of water in the latter, and to insist that, otherwise, the water on the outside of the vessel must have lost its original character, and be different from that within: it is demanding that mercury, which, by its constitution, can at  $212^{\circ}$  only expand a certain quantity, compared with its own bulk in some given lower tempera-

ture, should be able (without our making any comparative experiments to determine the point) to inform us how many times water will be increased its own bulk when we pour a greater quantity of heat into it!

The heat in the steam is as much sensible or free heat as it was before it passed into the steam, if these terms are to be applied to heat cognisable by our senses, and that may be measured comparatively. But *the steam is really its own thermometer*; and it indicates as truly the quantity of heat that has passed into a given quantity of water, as the thermometer does the quantity that passes into itself; aye, and by the same means too—the magnitude of its own volume.

Instead of supposing, in the case of steam, that heat has become latent, or been *changed*, would it not be more correct to ascribe the phenomenon that led to this idea to another cause, *a change in the form of the water*, which, by its constitution, is forced to become vapour, under the *common pressure* of the atmosphere, *whenever a certain number of times its own bulk of heat is poured into it*. The quantity, after proper comparative experiments, could then be expressed in sensible terms, and would turn out to be *the whole bulk of the steam, minus the original volume of the water in the compound*.

If some such method were followed, it appears to me extremely probable that we should soon arrive at many truths respecting the operations of the universally diffused substance *heat*, which otherwise must escape us, though the facts that might lead to them are daily presenting themselves in almost every chemical process. It would surely tend much to the advancement of science, if *the bulk, mass, or volume* of heat necessary to convert different solids into liquids, and liquids into gases, *under a given pressure*, were accurately determined by experiment. The thermometer would then be a more useful instrument than it now is—But we should never look to it to perform impossibilities; we should no more expect it to measure the quantity of heat passing into or out of bodies, than we should attempt to measure the quantity of water delivered from a pump, by placing an hygrometer or any twisted fibrous substance in the stream, and then examining



mining how much its length is diminished or its diameter increased.

Some may say, that the case of the conversion of water into steam is not held by them as one of those that prove the passing of heat from a sensible to a latent state, and that therefore our reasoning, drawn from that example, will not invalidate the doctrine; “for in the case of water they consider the heat as in simple mixture, and it would be an abuse of words to call so weak an union by the name of combination.”

I reply, that this case was one of the earliest brought in support of the doctrine, and also thought to be one of the strongest; and if the good understanding of any has led them to give it up, it is the more surprising they should be so blind as to continue upholding a fabric which was built on this as one of its foundation stones; and which does not appear to me to be upheld by any one fact that may not be as satisfactorily explained, without admitting the existence of heat in two distinct states.

But, say these, two different fluids of the same temperature when united will often give out heat—What can we say of this heat, but that it was *latent* or *combined* in one or both of the fluids, and that it is thrown out as sensible or free heat by their union? I would say no such thing without a previous examination of all the accompanying phænomena; and one of these I find to be *a reduction in the volume of the compound*, which is less than that of the sum of the two. The *moleculæ* of the two substances occupy less room united than in their respective fluids. For instance, when sulphuric acid and water are joined, the volume of the mixture is less than that of the two before mixture; there is, therefore, less lodging-room left for the heat: in other words, the capacity of the compound for heat is less than the sum of the capacities of the ingredients: therefore, compared with surrounding bodies, it has now too large a quantity, and, by the law of equilibrium, must give off the surplus to the surrounding bodies in proportion to their capacities, reserving of such surplus only that portion due to itself, and necessary to give it such an increased temperature as the surrounding bodies

will each have acquired, by the diffusion of the dislodged heat, when it has come again to a state of equilibrium; a quantity which must in general be so small as to elude all measurement in the petty processes of the laboratory.

Are there any cases in which heat is dislodged by the union of two liquids, and where, at the same time, the volume of the mixture is not reduced below that of the sum of the volumes of the ingredients? I do not recollect any. There may, however, be some, and it will be time enough to attempt to explain them when they are adduced. In the mean time, when a diminution of volume follows, or rather accompanies, the extrication of heat from any body, instead of running to the doctrine of latent heat being then made sensible (that is, heat being changed in its character), we ought to content ourselves with stating an obvious fact, namely, that the molecularæ of the two liquids are so constructed and formed as to admit of their coming closer together when mixed than they could when respectively alone; and, of course, now fill reciprocally spaces that, before their mixture, were filled with heat: the latter substance, in consequence of being thus dislodged (for two substances cannot, at one and the same time, occupy the same space), diffuses itself among the surrounding bodies in proportion to their capacities, constituting in them, what it did in those it has quitted, *bulk* or *volume*.

What takes place in such cases may be illustrated by one of a different kind. If a pint of small shot and a pint of dry sand be mixed, they will occupy a less volume than two pints; air is thrown out that was before lodged in the interstices of both of them. Was it *latent air* then, and is it *sensible air* now? Did it differ in its properties before and after being ejected? Weighing the ingredients before and after mixture, will not tell how much air has been ejected; but we know, notwithstanding, that its volume may be measured; and so may that of the heat driven out in the case which this was brought to illustrate.

How are fluids in general measured but by their bulk? or their weight, when circumstances will allow it? Water, for instance, in common cases, is directly measured by any vessel



vessel whose capacity is known or can be come at; in others, as in a wet piece of wood, a brick, or other substance, the quantity must be found out by other means: but in every case where water is added to another substance, which is made thereby to expand exactly in the direct ratio of the quantity of water added (if there be any such), the quantity may be determined by measuring the compound, and deducting therefrom the original volume of the other substance. *Would it be absurd to talk of measuring the matter, fluid, or substance, called heat, in a similar manner?*

When a thermometer is applied to any substance of a higher temperature than itself, it is, by the operation of the general law, soon brought into equilibrium with that substance as to heat; and we say, "it has risen so many degrees." We are habituated to this mode of speaking, and satisfy ourselves, without any more inquiry, that the phenomenon requires no further investigation; and as to the accompanying phenomena, we generally overlook them altogether. When we find that the mercury has increased in volume, would it be absurd to ask this simple question? Is the increase to be attributed not merely to the addition of heat, but to the addition of a quantity *equal in bulk to the increase of volume acquired by the mercury?* I think it extremely probable that the amount of increase or diminution of the volume of any substance, when heat is added or abstracted, is the real bulk of the heat so added or abstracted. That I may be clearly understood, I shall illustrate my meaning by a comparison:

If to a cubic inch of a compact piece of gum and water new unknown quantities of water be added, who would ever think of wire-drawing the mixture through a tube, and expressing the result in degrees of no known quantity, nor referable to any determinate measure? The cases to me appear perfectly parallel.

To a mixture of gum and water we add water, and the volume of the mixture is increased; and to a mixture of mercury and heat we add heat, and the volume of that mixture is increased.

When

When the volume of a substance is increased by heat, can any thing be conceived more easily practicable, in many cases, than to determine, by actual measurement, the proportion that the increase bears to the volume of the mass at a given lower temperature? Is not this already done in many cases? The experiments on this point should be multiplied, so as to embrace, if possible, every known substance and every degree of heat. Most substances would then become their own thermometers: nay, all are so at present, but we have not examined the relations of all their different scales.

[To be continued.]

VI. *Experiments on the Solar and on the Terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different.* By WILLIAM HERSCHEL, LL.D. F.R.S.

[Continued from Page 21.]

5th Experiment. *Reflection of the Heat of a Coal Fire by a plain Mirror.*

I PLACED a small speculum, such as I use with my 7-feet reflectors, upon a stand, and so as to make an angle of 45 degrees with the front of it\*. This was afterwards to face the fire in my parlour chimney, and would make the same angle with the bars of the grate. At a distance of  $3\frac{1}{2}$  inches from the speculum, on the reflecting side of it, was placed the thermometer No. 1; and close by it, but out of the reach of the reflected rays, the thermometer No. 4. The whole was guarded in front, against the influence of the fire, by an oaken board  $1\frac{1}{2}$  inch thick, which had a circular opening of  $1\frac{1}{4}$  inch diameter, opposite the situation of the plain mirror, in order to permit the fire to shine upon it. The thermometers were divided from the mirror by a wooden partition, which also had an opening in it, that the reflected

\* See Plate II. fig. 2.



rays might come from the mirror to No. 1, while No. 4 remained screened from their influence. On exposing this apparatus to the fire, I had the following result :

	No. 1.	No. 4.	} Here, in five minutes, the heat reflected from the plain mirror raised the thermometer No. 1, 7 degrees; while the change in the temperature of the screened place, indicated by No. 4, amounted only to half a degree: which shows that an open fire sends out rays that are subject to the laws of reflection, and occasion heat.
0'	60	60	
1	62	60	
2	64	60	
3	65	60	
4	66	60	
5	67	60	} Here, in eleven minutes, the rays reflected by the prism raised the thermometer $4\frac{1}{2}$ degrees; but, the temperature of the place having undergone an alteration of $1\frac{3}{4}$ degrees, we can only place $2\frac{3}{4}$ to the account of reflection. The apparatus becoming now very hot, it would not have been fair to have continued the experiment for a longer time; but the effect already produced was fully sufficient to show, that even a prism, which stops a great many heat-making rays, still reflects enough of them to prove, that an open fire not only sends them out, but that they are subject to every law of reflection.

*6th Experiment. Reflection of Fire-Heat by a Prism.*

Every thing remaining arranged as in the 5th experiment, I removed the small plain mirror, and placed in its stead a prism which had one of its angles of 90 degrees, and the other two of 45 degrees each\*. It was put so as to have one of the sides facing the fire, while the other was turned towards the thermometer: the hypotenuse, consequently, made an angle of 45 degrees with the bars of the grate. The apparatus, after having been cooled some time, was exposed to the fire, and the following result was taken :

	No. 1.	No. 4.	} Here, in eleven minutes, the rays reflected by the prism raised the thermometer $4\frac{1}{2}$ degrees; but, the temperature of the place having undergone an alteration of $1\frac{3}{4}$ degrees, we can only place $2\frac{3}{4}$ to the account of reflection. The apparatus becoming now very hot, it would not have been fair to have continued the experiment for a longer time; but the effect already produced was fully sufficient to show, that even a prism, which stops a great many heat-making rays, still reflects enough of them to prove, that an open fire not only sends them out, but that they are subject to every law of reflection.
0'	$62\frac{1}{2}$	$62\frac{1}{2}$	
1	63	$62\frac{3}{4}$	
2	64	63	
4	$64\frac{1}{2}$	63	
5	65	$63\frac{1}{4}$	
8	$65\frac{3}{4}$	$63\frac{1}{2}$	} Here, in eleven minutes, the rays reflected by the prism raised the thermometer $4\frac{1}{2}$ degrees; but, the temperature of the place having undergone an alteration of $1\frac{3}{4}$ degrees, we can only place $2\frac{3}{4}$ to the account of reflection. The apparatus becoming now very hot, it would not have been fair to have continued the experiment for a longer time; but the effect already produced was fully sufficient to show, that even a prism, which stops a great many heat-making rays, still reflects enough of them to prove, that an open fire not only sends them out, but that they are subject to every law of reflection.
10	$66\frac{1}{2}$	$63\frac{3}{4}$	
11	67	$64\frac{1}{4}$	

\* See Plate II. fig. 2. F.

*7th Experiment. Reflection of invisible Solar Heat.*

On a board of about 4 feet 6 inches long, I placed at one end a small plain mirror, and at the other two thermometers \*. The distance of No. 1, from the face of the mirror, was 3 feet  $9\frac{3}{4}$  inches; and No. 2 was put at the side of it, facing the same way, but out of the reach of the rays that were to be reflected by the mirror. The colours of the prism were thrown on a sheet of paper, having parallel lines drawn upon it, at half an inch from each other. The mirror was stationed upon the paper; and was adjusted in such a manner as to present its polished surface, in an angle of 45 degrees, to the incident coloured rays, by which means they would be reflected towards the ball of the thermometer No. 1. In this arrangement, the whole apparatus might be withdrawn from the colours to any required distance, by attending to the last visible red colour, as it showed itself on the lines of the paper. When the thermometers were properly settled to the temperature of their situation, during which time the mirror had been covered, the apparatus was drawn gently away from the colours, so far as to cause the mirror, which was now open, to receive only the invisible rays of heat which lie beyond the confines of red. The result was as follows :

	No. 1.	No. 2.	} Here, in ten minutes, the thermometer No. 1 received four degrees of heat, reflected to it, in the strictest optical manner, by the plain mirror of a Newtonian telescope. The great regularity with which these invisible rays obeyed the law of reflection was such, that Dr. Wilson's sensible thermometer, No. 2, which had been chosen on purpose for a standard, and was within an inch of the other thermometer, remained all the time without the least indication of any change of temperature that might have arisen from straggling rays, had there been any such. I now took away the mirror, but left every thing else in the situation it was. The effect of this was thus :
0'	56	56	
—	57	56	
—	59	56	
7	60	56	
10	60	56	

\* See Plate IV. fig. 1.



	No. 1.	No. 2.	} Here, in ten minutes, the thermometer No. 1 lost again the four degrees it had acquired, while No. 2 still remained unaltered; and this becomes therefore a most decisive experiment, in proof of the existence of invisible rays, of their being subject to the laws of reflection, and of their power of occasioning heat.
0'	60	56	
5	58	56	
8	57	56	
10	56	56	

*8th Experiment. Reflection and Condensation of the invisible Solar Rays.*

I made an apparatus for placing the small steel mirror at any required angle\*; and, having exposed it to the prismatic spectrum, so as to receive it perpendicularly, I caused the colours to fall on one half of the mirror, which, being covered by a semicircular piece of pasteboard, would stop all visible rays, so that none of them could reach the polished surface. On the pasteboard were drawn several lines, parallel to the diameter, and at the distance of one-tenth of an inch from each other; that, by withdrawing the apparatus, I might have it at option to remove the last visible red to any required distance from the reflecting surface. In the focus of the mirror was placed the thermometer No. 2. I covered now also the other half of the mirror, till the thermometer had assumed the temperature of its situation. Then, withdrawing the apparatus out of the visible spectrum, till the last tinge of red was one-tenth of an inch removed from the edge of the pasteboard, and the whole of the coloured image thus thrown on the semicircular cover, I opened the other half of the mirror for the admission of invisible rays. The result was as follows:

No. 2.		} Here, in one minute, the thermometer rose 19 degrees. I covered the mirror.
In the Focus of invisible Heat.		
0'	61	
1	80	
2'	72	} Here, in three minutes, the thermometer fell 16 degrees. I opened the mirror again.
3	67	
4	64	

\* See Plate II. fig. 1.

## No. 2.

In the Focus of invisible Heat.

5'  
683  
88

}

Here, in two minutes, the thermometer rose 24 degrees. I covered the mirror once more.

7'

69—And, in one minute, the thermometer fell 19 degrees. Now, by this alternate rising and falling of the thermometer, three points are clearly ascertained. The first is, that there are invisible rays of the sun. The second, that these rays are not only reflexible, in the manner which has been proved in the foregoing experiment, but that, by the strict laws of reflection, they are capable of being condensed. And, in the third place, that by condensation their heating power is proportionally increased; for, under the circumstances of the experiment, we find that it extended so far as to be able to raise the thermometer, in two minutes, no less than 24 degrees.

*9th Experiment. Reflection of invisible Culinary Heat.*

I planted my little steel mirror upon a small board\*, and, at a proper distance opposite to it, I erected a slip of deal,  $\frac{1}{2}$  inch thick and 1 inch broad, in a horizontal direction, so as to be of an equal height, in the middle of its thickness, with the centre of the mirror. Against the side, facing the mirror, were fixed the two thermometers No. 2 and No. 3, with their balls within half an inch of each other, and the scales turned the opposite way. A little of the wood was cut out of the slip, to make room for the balls to be freely exposed. That of No. 2 was in the axis of the mirror, and the ball of No. 3 was screened from the reflected rays by a small piece of paste-board tied to the scale. The small ivory scales of the thermometers, with the slip of wood at their back, which, however, was feather-edged towards the stove, intercepted some heat; but it will be seen presently that there was enough to spare. When my stove was of a good heat, I brought the apparatus to a place ready prepared for it.

No. 2.

In the Focus.

No. 3.

Screened.

52  
9152  
53

}

Here we find that, in one minute, the invisible culinary heat raised the thermometer No. 2, 39 degrees; while

\* See Plate IV. fig. 2.



No. 3, from change of temperature, rose only one, notwithstanding its exposure to the stove was in every respect equal to that of No. 3, except so far as relates to the rays returned by the mirror; and therefore the radiant nature of these invisible rays, their power of heating bodies, and their being subject to the laws of reflection, are equally established by this experiment.

*10th Experiment. Reflection of the invisible Rays of Heat of a Poker, cooled from being red-hot till it could no longer be seen in a dark Place.*

The great abundance of heat in my last experiment would not allow of its being carried on without injury to the thermometer, the scale of which is not extensive; I therefore placed a poker, when of a proper black heat, at 12 inches from the steel mirror\*, and received the effect of its condensed rays upon the thermometer No. 2, placed in the focus. Then, alternately covering and uncovering the mirror, one minute at a time, the effect was as follows:

			No. 2.	} Here, in six minutes, we have a repeated result of alternate ele- vations and depres- sions of the thermo- meter, all of which confirm the reflexi- bility, the radiant
The mirror covered	0'	61		
Open	-	68		
Covered	-	61		
Open	-	64		
Covered	-	59		
Open	-	61 $\frac{1}{2}$		
Covered	-	58		

nature, and the heating power of the invisible rays that came from the poker.

From these experiments it is now sufficiently evident, that in every supposed case of solar and terrestrial heat, we have traced out rays that are subject to the regular laws of reflection, and are invested with a power of heating bodies; and this independently of light. For, though, in four cases out of six, we had illuminating as well as heating rays, it is to be noticed that our proof goes only to the power of occasioning heat, which has been strictly ascertained by the thermometer. If it should be said, that the power of illuminating objects of these same rays is as strictly proved by the same experiments,

\* See Plate I. fig. 1.

I must remark that, from the cases of invifible rays brought forward in the four laft experiments, it is evident that the conclufion that rays muft have illuminating power becaufe they have a power of occafioning heat is erroneous; and, as this muft be admitted, we have a right to ask for fome proof of the affertion, that rays which occafion heat can ever become vifible. But, as we fhall have an opportunity to fay more of this hereafter, I proceed now to investigate the refraction of heat-making rays.

*11th Experiment. Refraction of Solar Heat.*

With a new ten-feet Newtonian telescope, the mirror of which is 24 inches in diameter of polifhed furface, I received the rays of the fun; and, making them pafs through a day-piece with four lenses, I caufed them to fall on the ball of the thermometer No. 3, placed in their focus. Thofe who are acquainted with the lines in which the principal rays and pencils move through a fet of glaffes, will eafily conceive how artfully, in our prefent inftance, heat was fent from one place to another—heat croffing heat, through many interfe&ing courfes, without jofling together, and each parcel arriving at laft fafely to its deftined place. As foon as the rays were brought to the thermometer, it rofe, almoft inftantly, from 60 degrees to 130; and, being afraid of cracking the glaffes, I turned away the telescope. Here the rays, which occafioned no lefs than 70 degrees of heat, had undergone eight regular fucceffive refractions; fo that their being fubject to its laws cannot be doubted.

*12th Experiment. Refraction of the Heat of a Candle.*

I placed a lens of about 1,4 inch focus and 1,1 inch diameter, mounted upon a fmall fupport, at a diftance of 2,8 inches from a candle\*; and the thermometer No. 2, behind the lens, at an equal diftance of about 2,8 inches; but which ought to be very carefully adjusted to the fecondary focus of the candle. Not far from the lens, towards the candle, was a pafteboard fcreen, with an aperture of nearly the fame fize as the lens. The fupport of the lens had an

\* See Plate IV. fig. 3.



eccentric pivot, on which it might be turned away from its place, and returned to the same situation again, at pleasure. This arrangement being made, the thermometer was for a few moments exposed to the rays of the candle, till it had assumed the temperature of its situation. Then the lens was turned on its pivot so as to intercept the direct rays which passed through the opening in the pasteboard screen, and to refract them to the focus, in which the thermometer was situated.

	No. 2.	} Here, in three minutes, the thermometer received $2\frac{1}{8}$ degrees of heat, by the refraction of the lens. The lens was now turned away.
o'	53 $\frac{7}{8}$	
1	55 $\frac{1}{2}$	
2	55 $\frac{3}{4}$	
3	56	

		} Here, in three minutes, the thermometer lost $2\frac{1}{8}$ degrees of heat. The lens was now returned to its situation.
o'	56	
1	54 $\frac{5}{8}$	
2	54 $\frac{7}{8}$	
3	53 $\frac{7}{8}$	

		} And, in three minutes, the thermometer regained the $2\frac{1}{8}$ degrees of heat. A greater effect may be obtained by a different arrangement of the distances. Thus, if the
o'	53 $\frac{7}{8}$	
1	54 $\frac{3}{4}$	
2	55 $\frac{5}{8}$	
3	56	

lens be placed at 3<sup>1</sup> inches from a wax candle, and the thermometer situated, as before, in the secondary focus, we shall be able to draw from 5 to 8 degrees of heat, according to the burning of the candle, and the accuracy of the adjustment of the thermometer to the focus. The experiment we have related shows evidently that rays invested with a power of heating bodies issue from a candle, and are subject to laws of refraction, nearly the same with those respecting light.

*13th Experiment. Refraction of the Heat that accompanies the coloured Part of the Prismatic Spectrum.*

I covered a burning lens of Mr. Dollond's, which is nearly 9 inches in diameter, and very highly polished, with a piece of pasteboard, in which there was an opening of a sufficient size to admit all the coloured part of the prismatic spectrum\*. In the focus of the glass was placed the thermometer No. 3; and, when every thing was arranged properly, I covered the

\* See Plate IV. fig. 4.

lens for five minutes, that the thermometer might assume the temperature of its situation. The result was as follows :

		No. 3.	} Here, in one minute, the thermometer received 112 degrees of heat,
The lens covered	o'	64	
Open	I	176	

which came with the coloured part of the solar spectrum, and were refracted to a focus ; so that, if the coloured rays themselves are not of a heat-making nature, they are at least accompanied with rays that have a power of heating bodies, and are subject to certain laws of refraction, which cannot differ much from those affecting light.

[To be continued.]

VII. *An Examination of ST. PIERRE'S Hypothesis respecting the Cause of the Tides, which, in opposition to the received Theory, attributes them to supposed periodical Effusions of the Polar Ices.* By SAMUEL WOODS, Esq. Read before the Askefian Society November 5, 1799.

THE tides are two periodical motions actuating the ocean (called the flux and reflux, or ebb and flow), which succeed each other alternately at an interval of about six hours ; the period of a flux and reflux being, upon an average, 12 hours 24 minutes, the double of which, 24 hours 48 minutes, corresponds to that of a lunar day, or the time elapsing between the moon's passing a meridian and coming to it again. These alternate elevations and depressions of the ocean so exactly correspond with the course of the sun and moon, as to time and quantity, that the influence of those luminaries has in all ages been considered as the cause of their production ; but it was reserved for modern times to ascertain the principle of their laws, and to calculate, with precision, the effects produced by the different situations of the sun and moon, and the proportions of their power. This principle is no other than gravitation. It is evident that, if the earth were entirely fluid and quiescent, its particles, by their mutual gravity, would form the whole mass into a perfect sphere : now, if any power be supposed to act on all the particles of this sphere



with equal force, and in parallel directions, the whole mass would be moved together without experiencing any alteration in its figure. But this is not the case with respect to the moon's action on our globe: the power of gravity diminishes as the square of the distance increases, and therefore the waters (at Z, Plate V. fig. 1.) on the side of the earth (A,B,C,D) next the moon (M), are more attracted by the moon than the central parts of the earth (O), and the central parts more attracted than the waters on the opposite side (at n); and therefore the distance between the earth's centre and the waters on its surface under and opposite to the moon will be increased. For, suppose three bodies in the same line (H,O,D), if they are all equally attracted by any power (as M), they will all move towards it with equal rapidity, their mutual distances continuing the same; but if the attraction of this power (M) is unequal, the body most forcibly attracted will move fastest, and their reciprocal distances will be proportionally increased: thus, the power of gravitation acting unequally on the three bodies (H,O,D), the distance of the first (H) from the second (O), and of the second from the third (D), will be increased in proportion to the difference of the gravitating power at the distance of the three bodies (H,O,D) respectively: now, suppose a number of bodies (ABCD) placed round the centre (O) so as to form a fluid ring, unequally attracted by some power (M); the parts nearest and furthest (H and D) from this power will have their distance from the centre (O) increased, while the sides of this ring (B and F), being nearly equidistant from the power (M), the centre (O) will not recede, but rather approach the centre (O), and form an ellipsis (nLIzN). To apply this reasoning to the case under consideration, while the earth, by its gravity, tends toward the moon, the water directly below her will swell and rise gradually; the water on the opposite side will recede from the centre (or, more properly, the centre will advance), and rise, or appear to rise, while the water at the sides is depressed, and falls below the former level: hence, as the earth revolves on its axis from the moon to the moon again in 24 hours 50 minutes, there will be two tides of ebb and two of flood in that period. In con-

sequence

sequence of the earth's motion on her axis, the most elevated part of the water is carried beyond the moon in the direction of the rotation, and continues to rise after it has passed directly under the moon, not attaining its greatest elevation till it has got about half a quadrant further. It continues also to descend, after it has passed at 90° distance from the point below the moon, to a like distance of about half a quadrant; and therefore in open seas, where the water flows freely, the time of high water does not exactly coincide with the time of the moon's coming to the meridian, but is some time after. Besides, the tides do not always answer to the same distance of the moon from the meridian, since they are variously affected by the sun's action, which brings them on sooner when the moon is in her first and third quarters, and keeps them back later when she is in her second and fourth: because, in the former case, the tide raised by the sun alone would be earlier than the tide raised by the moon: in the latter case, later.

We have hitherto considered the moon as the principal agent in producing tides, but it is obvious that the inequality of the sun's action must produce a similar effect; so that, in reality, there are two tides every natural day occasioned by the sun, as well as two tides every lunar day occasioned by the moon, and subject to the same laws: on account, however, of the sun's immense distance, his action is considerably inferior to that of the moon. By comparing the spring and neap tides at the mouth of the Avon, below Bristol, Sir Isaac Newton calculates the proportion of the moon's force to the sun's as 9 to 2 nearly. Dr. Horsley, in his edition of the Principia, estimates it as 5,0469 to 1; and, considering the elevation of the waters by this force as an effect similar to the elevation of the equatorial above the polar parts of the earth, it will be found that the moon is capable of producing an elevation of about ten feet, the sun of about two feet; which corresponds pretty nearly to experience.

In order to understand the cause of spring and neap tides, we must consider, that the moon, revolving round the earth in an elliptic orbit, approaches nearer and recedes further from it, than her mean distance, in every revolution or lunar month. When nearest, her attraction is strongest, and *vice*

*versa* :



*Versâ*: when both luminaries are in the equator, and the moon in perigeo, the tides rise highest, particularly at opposition and conjunction: at the change, when the attractive forces of the sun and moon are combined, the tide is raised to a greater height: at the full, when the moon raises the tide under and opposite to her, the sun, acting in the same line, raises the tides under and opposite to him, whence their conjunct effect is the same as at the change, and in both cases occasions what we call spring tides: but at the quarters, the sun's action diminishes the effect of the moon's action, so that they rise a little under and opposite the sun, and fall as much under and opposite the moon, these two luminaries acting obliquely on each other, and producing what is called neap tides \*.

The spring tides, however, do not happen precisely at the full and change of the moon, nor the neap tides at the quarters, but about two days later. In this, as in many other cases, the effects are not greatest, or least, when the immediate influence of the cause is greatest or least: as, for instance, the greatest heat of summer is not at the time of the solstice, but some weeks after; and if the actions of the sun and moon should be suddenly suspended, the tides would continue for some time in their usual course. The variations of the moon's distance from the earth produce a sensible difference in the tides. When the moon approaches the earth, her action on every part increases, and the differences of her action increase in a higher proportion as the moon's distances decrease. According to Sir Isaac Newton, the tides increase as the cubes of the distances decrease; so that the moon, at half her distance, would produce tides eight times as great. The sun being nearer the earth in winter than in summer, the spring tides are highest, and the neap tides lowest, about the time of the equinoxes, a little after the autumnal and before the vernal; and, on the contrary, the spring tides lowest and the neap tides highest at the solstices, when the

\* In Fig. 2. Plate V. H Z O N represents the earth; A B C D the moon's orbit. At the full and change, the sun and moon act in the same line S P: at the quarters, the sun's influence in the line S O H counteracts that of the moon acting in the direction M Z N, and produces neap tides.

sun is most distant from the equator. When the moon is in the equator, the tides are equally high in both parts of the lunar day; but as the moon declines towards either pole, the tides are alternately higher and lower at places having north or south latitude: while the sun is in the northern signs, the greater of the two diurnal tides in our climates is that arising from the moon above the horizon: when the sun is in the southern signs, the greatest is that arising from the moon below the horizon. Thus the evening tides in summer are observed to exceed the morning tides, and in winter the morning tides exceed the evening tides: the difference at Bristol is found to be 15 inches, at Plymouth 12. It would be still greater, but that a fluid always retains an impressed motion for some time, and consequently the preceding tides always affect those that follow.

If the earth were covered all over with the sea to a great depth, the tides would be regularly subservient to these laws; but various causes combine to produce a great diversity of effect, according to the peculiar situation and circumstances of places, shoals, fords, and straits: thus, a slow and imperceptible motion of a large body of water, suppose two miles deep, will be sufficient to elevate its surface ten or twelve feet in a tide's time; whereas, if the same quantity of water is forced through a narrow channel forty or fifty fathoms deep, it produces a very rapid stream, and of course the tide is found to set strongest in those places where the sea grows narrowest, the same quantity of water being constrained to pass through a smaller passage, as in the straits between Portland and Capela Hogue in Normandy; and it would be still more so between Dover and Calais, if the tide coming round the island did not check it.

The shoalness of the sea and the intercurrent continents are the reasons why the tides in the open ocean rise but to very inconsiderable heights, when compared to what they do in wide-mouthed rivers opening in the direction of the stream of the tide; and that high water is some hours after the moon's appulse to the meridian, as it is observed upon all the western coast of Europe and Africa from Ireland to the Cape of Good Hope; in all which a south-west moon makes  
high



high water; and the same is said to be the case on the western coast of America: so that tides happen to different places at all distances of the moon from the meridian, and consequently at all hours of the day.

To allow the tides their full motion, the space in which they are produced ought to extend from east to west  $90^{\circ}$  at least; such being the distance between the places most raised and depressed by the moon's influence. Hence it appears that such tides can only be produced in large oceans, and why those of the Pacific exceed those of the Atlantic ocean: hence also it is obvious why the tides in the torrid zone between Africa and America, where the ocean is narrower, are exceeded by those of the temperate zones on either side: and hence we may comprehend why the tides are so small in islands at a great distance from the shores, since the water cannot rise on one shore without descending on the other: so that at the intermediate islands it must continue at a mean height between its elevations on those shores.

The tide produced on the western coast of Europe corresponds to this theory. Thus, it is high water on the western coasts of Ireland, Spain, and Portugal, about the third hour after the moon has passed the meridian; from thence it flows into the adjacent channels, as it finds the easiest passage. One current, for example, runs up by the south of England, and another by the north of Scotland; taking considerable time to move all this way, and occasioning high water sooner in the places at which it first arrives, and begins to fall at these places while the current is proceeding to others further distant in its course. On its return it is unable to raise a tide, because the water runs faster off than it returns, till, by the propagation of a new tide from the ocean, the current is stopt, and begins to rise again. The tide propagated by the moon in the German ocean, when she is three hours past the meridian, takes twelve hours to come from thence to London bridge; so that, when it is high water there, a new tide has already attained its height in the ocean, and in some intermediate place it must be low water at the same time. When the tide runs over shoals, and flows upon flat shores, the water

is elevated to a greater height than in open and deep oceans that have steep banks, because the force of its motion is not broken upon level shores till the water has attained a greater height. If a place communicates with two oceans, or by two different openings with the same ocean, one of which affords an easier and readier passage than the other, two tides may arrive at this place in different times, which, interfering together, may produce a great variety of phænomena.

At several places it is high water three hours before the moon comes to the meridian; but that tide which the moon drives, as it were, before her, is only the tide opposite to that produced by her when nine hours past the opposite meridian.

It would be tedious to enumerate all the particular solutions easily deducible from these doctrines: as, why lakes and seas, such as the Caspian and the Mediterranean, the Euxine and the Baltic, have little or no sensible tides; since, having no communication, or being connected by very narrow inlets with the great ocean, they cannot receive or discharge water sufficient to alter their surface sensibly. In general, when the time of high water at any place is mentioned, it is to be understood on the days of new and full moon: the times of high water in any place fall at nearly the same hours after a period of about fifteen days, or between one spring tide and another.

This theory, however, is not without objections and difficulties; which has encouraged a Frenchman of some eminence, St. Pierre, to frame a new and singular hypothesis, ascribing all the phænomena of the tides to the periodical effusions of the polar ices. I shall first mention the most material facts and considerations which appear to militate against the common theory, as stated by St. Pierre; and I shall then endeavour to explain the theory he has substituted (which it has cost me some pains to collect, abstract, and arrange), as nearly as possible in a literal translation of his own language.

IT is said that, if the moon acted by her attraction, her influence must extend to the Mediterranean, the Baltic, the Cas-



pian, and the vast lakes of North America, in some degree at least; but all these have no sensible tides\*. This tranquillity renders her attraction liable to suspicion; and we shall, perhaps, find that the greatest part of the tides in the ocean have nothing more than an apparent relation either to her influence or her course.

The phases of the moon do not correspond all over the globe with the movements of the seas. On our coasts the flux and reflux follow the moon rather than her real motion: in various places they are subject to different laws, which obliged Newton to admit (chap. 25.) “that in the periodical return of the tides there must be some other mixed cause, hitherto undiscovered.”

The currents and tides in the vicinity of the polar circle come from the pole, as appears from the testimony of Fred. Martens, who asserts, that the currents amidst the ices set in towards the south; but adds, that he can state nothing with certainty respecting the flux and reflux of the tides.—*Voyage towards the North Pole*, 1671.

Henry Ellis observed that the tides in Hudson’s bay came from the north, and were accelerated as the latitude increased. It is impossible these tides should come from the line or the Atlantic. He ascribes them to a pretended communication with the South sea. At Waigat’s straits these north tides run at the rate of eight or ten leagues an hour. He compares them to the sluice of a mill.—*Voyage to Hudson’s Bay*, 1746.

Linscotten, in 1594, made nearly the same remarks, and observes that in Waigat’s straits the water was only brackish. He says the tides come from the east with great velocity, bringing with them large islands of ice.

W. Barents (1595) confirms this account.

All these effects can be produced by nothing else than the effusion of ices surrounding the pole. These ices, which melt and flow with such rapidity in the northern parts of America and Europe about the months of July and August,

\* The Caspian sea is about 860 miles long, and, in one place, 260 miles broad: there are strong currents, but no tides.

There is no regular flux and reflux in the Baltic.

In some particular spots of the Mediterranean there is a small tide.

greatly contribute to our high equinoctial tides; and when these effusions cease in October, our tides begin to diminish.

If the tides depended on the action of the sun and moon on the equator, they ought to be much more considerable towards the focus of their movements than any where else. But this is contrary to fact (Dampier says). From Cape Blanc, from the third to  $30^{\circ}$  south lat. the flux and reflux of the sea does not exceed two feet. The tides in the East Indies rise not above a foot; near the poles they rise 20 or 25 feet.

In the road of the island Massafuero ( $33^{\circ} 46'$  south lat.  $80^{\circ} 22'$  west long.) the sea runs twelve hours north, and then flows back twelve hours south: its tides, therefore, run towards the line.—*Byron, April 1765.*

At English Creek, on the coast of New Britain ( $5^{\circ}$  south lat.  $152^{\circ}$  west long.) the tide has a flux and reflux once in 24 hours.—*Carteret, Aug. 1767.*

At the Bay of Isles, in New Zealand ( $35^{\circ}$  south lat.); the tides set in from the south.—*Cook, Dec. 1769.*

At Endeavour River, in New Holland, neither the flood nor ebb tides were considerable, excepting once in 24 hours.—*June, 1770.*

At Christmas Harbour, in Kerguelen's Land, the flood came from the south-east, running two knots an hour.—*Cook, Dec. 1776.* It appears to have been regular and diurnal, *i. e.* a tide of twelve hours. The tide rises and falls about four feet.

At Otaheité the tides seldom rise more than twelve or fourteen inches; and it is high water nearly at noon, as well at the quarters as at the full and change of the moon.—*Cook, Dec. 1777.* It is evident, from a table of these tides for 26 days, that there was but one tide a day; and this, during the whole time, was at its mean height between  $\frac{1}{2}$  and 1. These tides, therefore, can have no relation to the phases of the moon.

Let us now take a cursory view of the effects produced by the tides in the northern part of the South sea. At the entrance of Nootka it is high water, on the days of new and full moon, at twenty minutes past twelve: the perpendicular rise and fall eight feet nine inches; which is to be understood of the

the



the day tides, and those which happen two or three days after the full and change. The night tides rise nearly two feet higher.—Cook, April 1778. These semidiurnal tides differ from ours in taking place at the same hour, and exhibiting no sensible rise till the second or third day after the full moon: all which is perfectly inexplicable on the lunar hypothesis.

These northern tides of the South sea, remarked in April, become, in higher latitudes, stronger in May, and still stronger in June; which cannot be referred to the moon's course then passing into the southern hemisphere, but must be ascribed to the sun's course passing into the northern hemisphere, and proceeding, as its heat increases, to fuse the ices of the north pole: besides, the direction of these northern tides towards the line constitutes a complete confirmation that they derive their origin from the pole.

At the entrance of Cook's River there was a strong tide setting out of the inlet at the rate of three or four knots an hour: higher up in the inlet, at a place four leagues broad, the tide ran with prodigious violence at the rate of five knots an hour. Here the marks of a river displayed themselves, the water proving considerably fresher.—Cook, May 1778.

What Cook calls a river, is nothing but a real northern sluice, through which the polar effusions are discharged into the ocean. Middleton (*Voyage to Hudson's Bay*, 1741 and 1742) found, between lat.  $65^{\circ}$  and  $66^{\circ}$ , a considerable inlet running west, which he calls Wager's River; and, after repeated trials of the tides for three weeks, found the flood constantly coming from the east. This is another of the northern sluices.

In Karakakooa Bay, Sandwich Islands, the tides are very regular, ebbing and flowing six hours each alternately.—Clerke, March 1779.

At the town of St. Peter and Paul, in Kamschatka, the tides are very regular every twelve hours.—Clerke, Oct. 1779.

Mr. Wales (*Introduction to Cook's last Voyage*) acknowledges that the tides observed in the middle of the great Pacific ocean fall short full two-thirds of what might have been expected from calculation.

The course of the tides towards the equator in the South  
sea;

sea; their retardations and accelerations on these shores; their directions, sometimes eastward, sometimes westward, according to the monsoons; finally, their elevation, which increases in proportion as we approach the poles, and diminishes in proportion to the distances from it, even between the tropics, demonstrate that their focus is not under the line. The cause of their motions depends not on the attraction or pressure of the sun and moon on that part of the ocean, for their forces would undoubtedly act there with the greatest energy, and in periods as regular as the course of the two luminaries.

Why, then, are the tides between the tropics so feeble and so much retarded under the direct influence of the moon?

Why does the moon, by her attraction, give us two tides every 24 hours in the Atlantic ocean, and produce only one in many parts of the South sea, which is incomparably broader?

Why do the tides take place there constantly at the same hours, and rise to a regular height almost all the year round?

Why do some rise at the quarters just the same as at the full and change?

Why are they always stronger as you approach the poles, and frequently set in toward the line, contrary to the principle of lunar impulsion?

These problems, which it is impossible to explain by the lunar theory, admit an easy solution on the hypothesis of the alternate fusion of the polar ices.—

Such are the most material objections adduced to invalidate the lunar theory. How far they are conclusive, shall be left to future investigation.

But St. Pierre is not content with demolishing the old structure; he has judged proper to erect a new one; and a fair exposition of this system will enable us to determine, by comparison, to which we shall give our suffrage.

It is well known that Sir Isaac Newton and Cassini differed in their opinion respecting the figure of the earth: the former conceiving it to be an oblate spheroid, flattened at the poles; the latter contending it must be oblong, or elongated at the poles.



poles. To ascertain this point, some of the most celebrated mathematicians of Europe were appointed to determine, by actual measurement, the length of a degree both at the equator and at the pole. They found that the polar degrees exceeded the equatorial, and concluded they must consequently be parts of a larger circle, and, of course, that the earth was flattened at the poles. This was universally considered as decisive of the question, till the genius of our Frenchman detected a gross and palpable error in the calculation, which had escaped their accurate knowledge and penetration: but, as the elongation of the poles constitutes a leading feature in the new theory, I shall give it a more detailed examination.

This polar elongation, as he conceives, is supported by four direct and positive proofs:—the first geometrical, upon which he lays the greatest stress, and upon which he has staked his reputation; the 2d, atmospherical; the 3d, nautical; the 4th, astronomical: of all which in order.

The 1st, or geometrical proof, is what he calls a demonstration founded on the measurement of the earth, and admitting the polar degrees to exceed the equatorial: here follows the demonstration: If you place a degree of the meridian at the polar circle on a degree of the same meridian at the equator, the first degree, which measures 57,422 fathoms, will exceed the 2d, which is 56,748 fathoms, by 674; consequently, if you apply the arc of the meridian contained within the polar circle, being  $47^{\circ}$ , to an arc of  $47^{\circ}$  of the same meridian at the equator, it would produce a considerable protuberance, its degrees being greater.

To render this more apparent, let us always suppose that the profile of the earth, at the poles, is an arc of a circle containing  $47^{\circ}$ ; is it not evident, if you trace a curve on the inside of this arc, as the academicians do when they flatten the earth at the poles, that it must be smaller than the arc within which it is described, being contained in it? And the more this curve is flattened the smaller it becomes. Of consequence, the  $47^{\circ}$  of this entire curve will be individually smaller than the  $47^{\circ}$  of the containing arc. But as the degrees of the polar curve exceed those of the arc of a circle, it must follow that the whole curve is of greater extent

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than the arc of a circle: now to be of greater extent it must be more protuberant: the polar curve, of consequence, forms a lengthened ellipsis. Q. E. D.\*

It must be acknowledged that this demonstration is very perspicuous and convincing. How the most celebrated academicians and mathematicians, for nearly half a century, could have overlooked a proposition so plain and simple, can only be ascribed, in the opinion of St. Pierre, to their obstinate and inveterate prejudices. He pursues his victory in a strain of vain and indecent exultation, which would dishonour a more respectable cause; but, perhaps, a little attention will induce us to doubt at least whether the charge of gross ignorance may not, with justice, be retorted on their accuser.

It would have been indeed extraordinary, if men of science had been absurd enough to imagine that a larger arc might be included in a less; but they might suppose, with propriety and justice, that the smaller arc of a larger circle can be included in the larger arc of a smaller circle, which, in the present instance, appears to be the case. In measuring a degree on the meridian, a certain spot is fixed upon, where the elevation of the polar star is taken by a quadrant; from this spot they proceed in a direct line north, till the quadrant indicates an additional elevation of one degree. In proportion as this degree constitutes a part of a larger or smaller

\* Let  $x$ , Fig. 3. Plate V. be the unknown arc of the meridian, comprehended above the arctic circle  $ABC$ ; and let  $DEF$  be the arc of the same meridian, comprehended between the tropics; these two arcs are each  $47^\circ$ . According to our astronomers, a degree at the polar circle is greater by 674 fathoms than a degree of the same meridian near the equator; the arc  $x$  therefore exceeds, in extent, the arc  $DEF$  by  $674 \times 47$ , or 31,678 fathoms  $= 12\frac{2}{3}$  leagues. The question to be determined is, whether this unknown polar arc  $x$  is contained within the circle in the curve  $AbC$ , or coincides with it, as  $ABC$ , or falls without its circumference, as  $A \neq C$ . The arc  $x$  cannot be contained within the circle, as  $AbC$ , for it would then be evidently smaller than the arc  $ABC$ ; and the more this curve  $AbC$  is flattened, the less will be its extent, as it will approach nearer and nearer to the straight line  $AC$ ; neither can it coincide with the arc  $ABC$ , for it exceeds it  $12\frac{2}{3}$  leagues. It must belong, therefore, to a curve falling without the circle, as  $A \neq C$ . The globe of the earth, therefore, is lengthened at the poles, since degrees of the meridian are greater there than at the equator.

circle,



circle, a greater or less portion of ground will be passed over before the desired elevation is observed; and the measurement of this ground unequivocally decides whether this degree is part of a larger or smaller circle. In this case the measurement is admitted, but the conclusion denied. St. Pierre seems to have supposed, that the academicians divided the polar arc into 47 parts, and then measured one of these parts: a thing impracticable and ridiculous. The fact is, that the polar arc, which, if the earth were a perfect sphere, would contain  $47^\circ$ , does not actually contain so many, but perhaps about  $46^\circ$  of a larger circle; and if the polar degrees are parts of a larger circle, as they certainly are, it is demonstrably evident that the real arc must be contained within the spherical arc, and, consequently, that the earth is flattened at the poles \*.

[To be continued.]

\* Let the circle  $A B C D$ , Fig. 4. represent the earth as a sphere, and let  $P$  represent the polar star, having no sensible parallax. Draw the diameter  $B D$ , prolonging it to  $P$ ; draw the transverse diameter  $C A$ , the tangent  $A P$ , and the line  $F P$ , parallel to  $A P$ ; bisect the quadrant  $A D$  equally at  $F$ ; draw the tangent  $K L$  perpendicular to the radius  $G F$ , and with the radius  $B F$  describe the circle  $E O M N$ , and let the segment  $H E F$  represent the earth flattened at the pole; draw the tangent  $S R$  to the circle  $E O M N$ , perpendicular to the radius  $B F$ . An observer at  $A$  will perceive the polar star  $P$  in the horizon; an observer at  $D$  or  $E$  will perceive it in the zenith, or at an elevation of  $90^\circ$ . If the earth be a sphere, the tangent  $K L$  will be the horizon to an observer at  $F$ , and the angle of elevation  $P F K$  is, by construction,  $45^\circ$ ; but if the earth is not a sphere, but flattened towards the pole, as in the segment  $H E F$ , the tangent  $S R$  will be the horizon to an observer at  $F$ , and of course the angle  $P F S$  will be the angle of elevation. Now, the angle  $E G F$  is by construction  $45^\circ$ , consequently, the  $\angle F G B = 135^\circ$ , and the angles  $G B F$  and  $G F B$  each  $22^\circ 30'$ . Draw the dotted line  $E F$ . Now the triangle  $B E F$  is an isosceles triangle, and the angle  $G B F$  being found  $= 22^\circ 30'$ , it follows that the angles  $B E F$  and  $B F E$  are each  $78^\circ 45'$ , and the  $\angle S F B$  being a right angle, the  $\angle S F E = 90^\circ - 78^\circ 45' = 11^\circ 15'$ . At  $E$  draw the tangent  $T V$ , and, for the same reason, the  $\angle V E F = 11^\circ 15'$ ; and consequently the  $\angle E x F = 157^\circ 30'$ , and the  $\angle V x F = 22^\circ 30'$ . Now the  $\angle F Z E$  is a right angle, therefore the angle  $P F S = 180^\circ - 90^\circ - 22^\circ 30'$ , or  $67^\circ 30'$ . The difference, therefore, between the elevation at  $E$  and at  $F$  will be  $90^\circ - 67^\circ 30' = 22^\circ 30'$ , but the difference between the elevation at  $F$  and  $D$  will be  $45^\circ$ ; whence it is evident that a larger measurement of ground will be included in a degree in proportion as  $H E F$  is the segment of a larger or smaller circle.

VIII. *Account of a new Operation lately performed with Success in France, for restoring Sight in certain Cases of Blindness.* By C. DEMOURS.

C. DEMOURS, who performed this new and ingenious operation, presented a memoir on the subject to the National Institute, which was read on the 15th of June last. From this memoir the present account has been extracted.

The eye is a ball or globe filled with different transparent humours, which are contained in several membranes. The outermost of these membranes is called the cornea; it is transparent, and placed before the iris, which is the coloured part of the eye, and which itself forms a second membrane; but the latter is not transparent, and would prevent the rays of light from penetrating to the hind part of the globe of the eye, were it not pierced with a round hole, called the pupil. A little beyond this hole is the crySTALLINE humour, forming a kind of lens, through which the rays of light are obliged to pass; and by means of which they form distinct images on the retina at the bottom of the eye. The humour contained in the inside of the globe is called the vitreous humour; it is perfectly transparent, as is also the aqueous humour, which is placed between the transparent cornea and the crySTALLINE.

C. Sauvages, on whom C. Demours performed the operation which is the subject of this article, had for several years an abscess in the cornea, in consequence of which the aqueous humour was entirely wasted, and the transparent part of the cornea had become totally white and opaque in the right eye, and for about four-fifths in the left eye. This eye is represented in the annexed engraving (Plate V. fig. 5). The transparent cornea, C, suffers to be seen towards the upper part but a small portion of the iris. The white spot covers entirely the round hole or pupil, which exists in the centre of the iris, and through which alone the rays of light can pass.

C. Demours, taking advantage of the transparency which still existed towards the upper part of the cornea, made there a small aperture, denoted by the letter A; and having introduced



roduced into that aperture a delicate pair of scissars, made a small hole in the iris, of the size of a seed of sorrel, indicated in the engraving by the letter D.

Through this hole, made in a non-transparent membrane, the rays of light now enter, and form images on the retina at the bottom of the eye. It is, therefore, an artificial pupil which supplies the place of that covered by the white spot of the cornea. But as behind this new pupil there is no crystalline humour to collect the rays with sufficient accuracy on the retina, C. Sauvages is obliged, in order to read, to make use of a very convex glass, such as is used by persons who have undergone an operation for the cataract, and who have lost the crystalline humour. He does not employ the glass, however, except in cases which require more distinct vision; and he still possesses the invaluable advantage of seeing well enough to direct his way, and to discern perfectly every object around him. By means of this ingenious operation, sight in future may be restored to the greater part of those who have lost that faculty by large spots or white scars, cases hitherto considered as incurable; provided that some part of the cornea, opposite to the iris, has remained transparent.

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IX. *On the Cultivation and Use of the Syrian Silk-Plant.*

By J. A. MÖLLER, Director of the Westphalian Patriotic Society\*.

I. *Description.* **T**HE silk-plant, *Asclepias Syriaca* LINN. known by the old botanists and our gardeners under the name of *Apocynum Syriacum*, came originally from Syria and Egypt. It is indigenous also in North America, and thrives so well in Europe, that it would appear that it is suited for all countries and climates. The root is perennial, and will last from ten to twenty years. In the month of April it throws out, like asparagus and hops, a great number of shoots, the principal stem of which rises to the height of seven or eight feet. This stem, which is as thick

\* From the *Transactions of the Academy of the Useful Sciences at Erfurt.*

as a man's finger, is straight, round, and smooth, and beset with oval leaves of considerable size, covered on the upper side with dark green, and on the lower side with whitish down. The plant has a milky juice, which is said to be perfectly harmless. The flowers appear about the end of May, and continue till the month of July; there are often from twelve to sixteen on one stem, each of which forms a bunch, containing from thirty to forty single flowers. Each single flower adheres to the bunch by a long thin stalk, and has a sweetish odour. Each bunch of flowers is succeeded by three, four, and sometimes ten long, flat, and rough pods, which inclose several round, yellowish brown, flat and thin seeds wrapped up in a beautiful shining white kind of silk. The seeds are winged; a form which nature has given with great variety to many others, in order that they may be conveyed with more ease, and to a greater distance, by the wind.

II. *Uses.*—1st, The silk, which covers the seeds in the pods, is the principal part of use, and that from which the whole plant takes its name. The pods gradually acquire maturity from August to the beginning of October, at which time those who cultivate this plant must watch with great care for the period of their bursting, in order to collect the silk, lest it should be carried away by the wind, or be spoilt by the rain. The pods when collected are spread out, to the height of about half a foot, on a net or rack, in an airy place, in order to dry.

The silk, which is of a shining white colour, from an inch to an inch and a half in length, and exceedingly elastic, is then taken out, and being freed from the seeds is hung up in thin bags in the sun, in order that it may become perfectly dry; and at the same time it is often softened with the hand, or by being beat. This vegetable silk may now be used, without any further preparation, instead of feathers and horse-hair, for beds, cushions, coverlets to beds, bolsters, and mattresses. From eight to nine pounds of it, which occupy the space of from five to six cubic feet, will be sufficient for a coverlet, bed, and two pillows; such beds therefore are exceedingly convenient for travelling. It is not advise-



able, however, to use this silk in common for beds instead of feathers, as it is too soft and warm. It requires as little preparation for quilts and counterpanes, and is lighter and warmer than those of common silk.

For spinning, however, notwithstanding its fineness, which approaches near to that of common silk, it is not fit when taken alone, as it is almost too short, and therefore must be used with an addition of flax, wool, or common silk, but particularly of cotton. One-third of this silk with two-thirds of cotton forms a very good mixture for gloves, stockings, and caps. Other mixtures may be used for different kinds of stuffs; but it has been observed that the cloth is much stronger when the vegetable silk is employed for the woof rather than for the warp. Many colours have been applied to such cloth with great success, but as each substance requires a peculiar mode of treatment, more experiments on this subject are necessary: a mixture of one-third vegetable silk, and two-thirds of hare's down, forms hats exceedingly light, and soft to the touch, which have a great resemblance to beaver hats, and are much cheaper.

2d, As soon as the pods have been collected, the stems, which contain a fibrous part capable of being spun, must be cut before they become dry or suffer from the night frosts. They must then be immersed for some days in water, like flax or hemp, and then dried by being spread out on the grass. Care, however, must be taken to ascertain, by experiments, the proper length of time, as too much or too little would be prejudicial. In the last case the flaxy part is brittle, and in the former it loses its strength. After it has been watered it is beaten and heckled: for beating it various kinds of machinery have been invented, and for bruising the stems particular mills are used.

A mixture of the threads spun from the flax of these stems with the vegetable silk and cotton, produces a kind of cloth very proper for furniture. It has been however employed chiefly, with and without an addition of rags, for making all kinds of writing and packing paper; which sometimes is similar to the Chinese paper, and sometimes exceeds in strength the usual paper made from rags.

3d, Both

3d, Both the inner white skin, and the external green husk of the capsules, which contain the seeds, might be employed for manufacturing the finer sorts of this silk paper.

4th, That as little as possible of this plant should remain useless, Nature has provided in the sweet juice of its flowers excellent nourishment for bees. According to the author of *Geschichte Meiner Bienen*, this plant, in the above respect, the lime-tree excepted, is superior to all other vegetable productions.

In America a kind of brown sugar is prepared from the juice of these flowers.

The great utility of this plant has been known only within these thirty years, though it is probable that it was introduced into Europe about the time of the crusades. A manufactory of articles from the silk of this plant has been established at Paris since 1760, and it has long been employed at Lausanne, with advantage, for making candle-wicks; but no one has shown more zeal in regard to the cultivation and preparation of this article than Mr. Schneider of Liegnitz, who has recommended it in two different pamphlets. In regard to the application of it to paper-making, Mr. Schmid of Lunenburg has made a variety of experiments; and it is much to be wished that others would imitate his example.

III. *Cultivation*.—This plant is propagated two ways, either by the seed or by slips. In the month of March, after the land has been well dug, the seeds are sown thin, and singly, in furrows of the depth of an inch, and covered with earth, which is thrown over them to the depth of half an inch: they are secured also from the night frost by moss or a little light dung. In from four to six weeks the young plants begin to appear. The first year they produce flowers, but do not bear seeds till the second, and do not come to full maturity till the third. In the third year they are transplanted. But this method is more laborious, and perhaps ought not to be recommended but in particular cases, such as when the roots have degenerated, or when they are transplanted to a different climate.

The object will be sooner accomplished by slips from the roots. As the plant throws out around it long roots with

new



new eyes, these must be lopped off from the old stock either in autumn, when the milky juice in the plant has dried up, or in the spring, before it again flows; and are to be cut into pieces of from four to six inches in length, but care must be taken that they have a sufficient number of eyes. A fresh incision must be made in the root before and behind, and they are then to be planted in the ground, to the depth of four or five inches, in an oblique position, with the eyes or buds standing upright. Those planted in autumn will produce seeds the next summer, and those planted in spring will bear the second summer.

In regard to the further care which this plant requires, the following observations deserve attention:—It thrives in almost every kind of soil, and even the most stony, without all further care: but, in order to be brought to perfection, it requires a tender sandy soil; which, however, must not be too poor and dry, and which ought to have as much sun as possible. In such soil, when properly nurtured, it produces the longest, finest, and most beautiful silk.

The ground, before it is planted, must be dug up to a good depth, and well dunged. It must also be well weeded, and kept exceedingly clean. After the crop has been collected, the stems must be cut close to the ground; and the plants which have died must be replaced by young ones. Towards winter they must be covered with a little dung, which ought to be spread in the spring. A sufficient space also must be left between the plants. They ought to be planted in rows, and at the distance of one foot and a half, or rather two feet, from each other.

Of the stems which shoot up, only the best (perhaps about one half) should be left standing; the rest, as soon as the flowers appear, should be cut, and placed in sand or earth, to dry up the milky juice that flows from them. Even of the prime plants, it will be proper to suffer four or five of the lowest bunches of flowers to come to maturity. By following these cautions, the silk obtained will be of a superior quality. The increase is very great. In the year 1785, Mr. Schneider began with six plants, and in 1793 had a plantation which contained 30,000. The first crop produced eight,

the second 356, and the third 600 pounds of silk. If the leaves, after the crop has been collected, be thrown together in heaps to rot, they form an excellent manure for future use. In regard to the preparation of the silk, little is necessary to be said. It may easily be conceived that it will be of advantage to separate that which is long from the shorter part, in order that the former may be employed for spinning. The shorter kind may be used for beds and for hat-making.

This plant may be employed also in the manufactory of paper. Schmid, an ingenious paper-maker near Lunenburg, has made several experiments with the capsules of this plant, which gave the following results :

1st, From the interior white rind of the capsule, mixed with one-third of rags, he obtained writing-paper pretty white, of a good quality, and similar to the silk paper of the Chinese.

2d, From the external green part of the capsules a greenish-coloured paper, which, when sized, was stronger than paper made of rags : it was almost as close in its texture as parchment, and even when unsized did not suffer the ink to penetrate through it. This kind was exceedingly proper for wrapping-paper.

3d, From the stems he obtained a paper so like in every thing to common paper made of rags, that the difference could scarcely be distinguished.

X. *On several new Properties discovered in phosphorised Hydrogen Gas.* By C. RAYMOND, *Professor of Chemistry in the Central School of Ardeche* \*.

WE are indebted to C. Gengembre for the interesting discovery of phosphorised hydrogen gas. No chemist before him had observed an elastic fluid which, like the one here spoken of, possesses the singular property of inflaming by the contact of the air alone, without requiring its temperature to be raised, or an ignited body to be presented to it.

\* From the *Annales de Chimie*, No. 105.



The kind of undulating and always increasing ring to which this gas gives birth; when made to burn bubble by bubble in a place where the air is perfectly calm; the splendour and magnificence which accompany its combustion when effected in oxygen gas perfectly pure; the sudden penetration of these two gases, and their total conversion into water and phosphoric acid; such were the only facts known which had been interesting to chemists in the history of phosphorised hydrogen gas, when I endeavoured to discover whether this elastic fluid did not possess other properties, which, if they did not exhibit so brilliant a spectacle as the greater part of those above alluded to, might be worthy of engaging the attention of chemists.

The property, long known, which sulphur communicates to hydrogen, and hydrogen reciprocally to sulphur, of being able to dissolve together in water, while both taken separately are perfectly insoluble, had already given me reason to think that the case might be the same with the combination of phosphorus and hydrogen; and that these two substances, being previously united, might perhaps then become susceptible of partaking in the liquidity of water, and communicate to it new properties nearly analogous to those possessed by that solution of sulphurated hydrogen gas known under the name of *hepatised water*.

To destroy or confirm this supposition, I took a glass bottle, and, having filled it with newly distilled water, inverted it on the shelf of a pneumatic tub, in order that I might introduce into it phosphorised hydrogen gas arising from the decomposition of water by a mixture made with phosphorus and fresh-slaked lime. When the bottle was half filled with phosphorised hydrogen gas I removed it from the tub, taking care to shut very closely its aperture with my finger, while I shook it with force in order to effect more speedily an union of the gas with the water, in the same manner as the solution of carbonic acid gas, as well as that of sulphurated hydrogen gas, is facilitated by making use of the same means.

I soon perceived, by the strong adhesion of my finger to the mouth of the bottle, that a considerable vacuum had been effected in it, and perhaps, even, that the whole of the phosphorised

phorised hydrogen gas in it had participated in the liquidity of the water.

I then removed my finger, but not without some difficulty, from the mouth of the bottle, that I might examine more accurately the nature and properties of the liquid it contained, and to ascertain whether there did not yet remain some portions of the gas susceptible of inflammation by the contact of the air. But, scarcely was the communication established between the atmosphere and the inside of the bottle, when a loud detonation took place, accompanied with a very brilliant light. I readily judged from this phænomenon, that the whole of the phosphorised hydrogen gas had not been absorbed by the water contained in the bottle; on which account I closed the aperture, in order to prevent the combustion from being longer continued; which would not have failed to produce a considerable quantity of phosphoric acid, and consequently to occasion great uncertainty in regard to the results which I wished to obtain.

The bottle having been closely stopped, I continued to shake it several times, under an idea that I should by these means be able to fix entirely the last portions of the phosphorised hydrogen gas which had remained undissolved!

Hoping that I had succeeded, and impatient to know the new properties which the water saturated with the gas might have acquired, I determined to unstop the bottle a second time in contact with the air. This was soon followed by a second detonation, stronger, indeed, than the former. After this I did not think of stopping the aperture of the bottle, so that a flame exceedingly pale continued for some minutes to issue from it.

When I observed no more apparent signs of combustion, I examined by the smell and taste the liquor remaining in the bottle. Its smell appeared to me exceedingly disagreeable, and altogether different from that of gaseous phosphorised hydrogen; its taste, though very bitter, had in it, however, something insipid and disgusting; and its colour was a little inclining to that of lemon.

When tried with tincture of turnsol it soon made it sensibly red, which I ascribed to the small portion of phosphoric acid  
which



which must have been produced at the moment when the detonations took place, as well as during the disengagement of the flame which followed the second detonation.

I was then obliged to begin a new operation, that is to say, to dissolve phosphorised hydrogen gas again in water, that I might ascertain, by employing more caution, in what accurate proportions this solution might be effected; and that I might prevent combustion also from taking place in the inside of the bottle, which would have become a source of error in the conclusion of my experiments.

I had ascertained with certainty, by means of the quantity of water and gas which I had tried to dissolve the first time, as well as by the detonations which had taken place at the moment when the bottle was unstopped, that distilled water could not dissolve at the temperature of  $47^{\circ}$  a volume of phosphorised hydrogen gas equal to its own. I took care, therefore, to introduce into the bottle the second time only about a third of its capacity of the gas. I then repeatedly shook the mixture in order to render the union of the gas with the water speedier, and as complete as possible; after which I unstopped the bottle, holding it inverted in a small tub, which I had filled with newly distilled water, that I might see whether it would become entirely filled with it merely by the effect of the pressure of the atmosphere, and that I might thence judge whether the whole gas had been liquefied.

I indeed saw a portion of the water in the tub ascend into the bottle, but I perceived also that it was not entirely filled; which confirmed me in the opinion that there still remained a portion of the gas which had not participated in the liquidity of the water. Having then tried to make some bubbles of it issue out through this liquid, these bubbles inflamed spontaneously by the contact of the air alone; which proved to me that the phosphorised hydrogen gas had not been decomposed at all by agitation, nor by its contact with distilled water; whereas it soon loses its highly combustible property when collected in bottles filled with water which has not been distilled, or distilled water which has been long kept, which I ascribe to the quantity of air which common water always holds

holds in solution, the oxygen of which, joining a portion of the phosphorus, effects its separation from the hydrogen by converting it into phosphorous oxyd, which being altogether insoluble, deposits itself on the sides of the vessel, without any appearance in this kind of oxydation of any sensible sign of combustion; while the oxygen of the atmospheric air, which participates in the liquidity of the water, being always in that case deprived of a large portion of the light and caloric which are combined with it in its aërial aggregation, cannot produce these two effects in a very sensible manner when it passes in this state of liquid aggregation from one combination to another.

Having again agitated the bottle after closing the mouth of it, I was able, by means of the small quantity of water which had been introduced into it, and of which I kept an exact account, to render the absorption of the last portions of the gas complete; for, having a second time uncorked the bottle in the same distilled water, I then saw it become entirely filled. I think I may affirm, therefore, from these trials, and some others which I made, that water freed from air by distillation can dissolve and liquefy, at the usual temperature, a little less than the fourth of its volume of phosphorised hydrogen gas, and that with this dose it is completely saturated.

This solution thus prepared, and kept free from the contact of the air, always exhibited the following properties:

In colour, it has a pretty near resemblance to sulphur in sticks, though a little less dark; it has a strong disagreeable odour, and an exceedingly bitter, nauseous, and disgusting taste.

When examined in the dark, this solution does not appear luminous; which proves that the phosphorus is intimately combined in it with the hydrogen.

When distilled in a small retort, connected with a pneumatic apparatus, it furnishes, at a temperature a little above that of boiling water, and especially when distilled soon after it has been prepared, a very large quantity of phosphorised hydrogen gas, as pure and as combustible as that obtained by heating caustic alkalies, or quicklime, with phosphorus and a very small quantity of water: what afterwards



wards remains in the retort after the disengagement of this gas has entirely ceased, is nothing else than pure water, having neither odour, taste nor colour, and perfectly resembling water newly distilled.

When brought into contact with atmospheric air, this solution soon suffers to be deposited a remarkable quantity of red oxyd of phosphorus, and at the same time suffers to be disengaged hydrogen gas, which is no longer susceptible of inflammation, except when brought into contact with a body in a state of ignition. If the solution be long exposed to the air, and if the points of contact be frequently renewed by agitation, it becomes completely decomposed; that is to say, is entirely resolved into phosphorus, oxyd, and pure hydrogen gas.

Tincture of turnsol and that of violets experience no change in their colour from being in contact with liquid phosphorised hydrogen; which proves that this liquor is neither acid nor alkaline.

The sulphuric and nitric acids, or the simple or oxygenated muriatic, when poured over this liquor, produce no effect worthy of notice.

Potash, soda and ammonia act in the same manner.

The oxyds of mercury and lead are speedily reduced, and immediately converted into metallic phosphurets, by being mixed with the solution of phosphorised hydrogen gas.

When poured into nitrat of silver, this solution immediately produces a very abundant black precipitate, which does not in the least change its colour, and which, when tried by the blow-pipe, exhibits the characters belonging to metallic phosphurets.

When brought into contact with a solution of mercury by nitric acid, it gives also immediately a very considerable precipitate, which has first a black colour, but which becomes white and crystallised in proportion as it passes from the state of phosphuret to that of mercurial phosphat, by imbibing oxygen, either from the nitric acid in which the precipitation takes place, or from the atmospheric air with which it is in contact.

A solution of lead by the nitric acid is also decomposed by

by the hydro-phosphorous liquor, but with less force than the solutions of silver and mercury are. There is formed in this decomposition also phosphuret of lead, which in the course of time is converted into a phosphat.

The fulphat of copper shows also, at the end of a certain time, a pretty abundant black precipitate when poured into a solution of phosphorised hydrogen. This precipitate, like that obtained by the decomposition of nitrat of silver, retains its colour; which may give us reason to believe that it can be converted only with difficulty into a phosphat.

Sulphat of iron did not appear to me to experience any sensible decomposition till the end of several days. Nitrat of arsenic, poured into this liquid, did not experience any sensible decomposition till the end of several days. There was then formed a precipitate of a very beautiful yellow colour under the form of small grains, and which could remain a long time exposed to the air without experiencing any kind of change. This precipitate was an arsenical phosphuret.

It results from these new properties, first discovered, I think, by myself, in phosphorised hydrogen gas, 1st, That this gas can unite itself to distilled water, in the proportion of about a fourth of its volume, when the solution is effected at the temperature of 44.5 of Fahrenheit's thermometer\*. 2d, That this gas communicates to the water in which it is dissolved a strong disagreeable odour, as well as a bitter taste, which may one day make it be employed with success in the treatment of many diseases, either on account of the facility with which this preparation suffers itself to be decomposed, or of the part performed by the phosphorus it contains in the formation of animal matters. 3d, That when water well freed from air has been employed to liquefy this gas, and when care has been taken to keep it thus dissolved in bottles well corked, it may be preserved a long time without experiencing decomposition, so that by heating the solution you may extract from it, in the state of gas, all the phosphorised hydrogen it contains. 4th, That when

\* It is probable that at the temperature of freezing water might dissolve a larger quantity, but the want of phosphorus prevented me from ascertaining this fact.



the water has thus been freed from all the phosphorised hydrogen it had dissolved, it becomes pure water; which proves that it was indebted for its new properties to the presence of this gas alone. 5th, In the last place, that this solution is capable of speedily reducing several metallic oxyds, whether alone or dissolved by acids, and of forming with them, by means of double elective attraction, water and metallic phosphurets, combinations which hitherto have been obtained only in the dry way; that is to say, by heating metals with phosphorus, or by decomposing phosphoric glass or metallic phosphats by metals and charcoal. Such are the properties which appear to me sufficiently interesting to be worthy of being added to the still imperfect history of phosphorised hydrogen gas.

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XI. *On the general Nature of Light.* By Mr. ROBERT HERON. *Communicated by the Author.*

**T**HROUGH the medium of the Philosophical Magazine I beg leave to lay before men of science the following opinions, which have lately suggested themselves to me as necessary and fair inductions from those facts that have come to my knowledge respecting light.

1. Light passes only through the vacuities in other bodies; does not penetrate their solid substance, so as ever to coexist with it in the same place.

2. It passes through those vacuities in straight perpendicular lines, without any loss of its qualities by attenuation and subdivision of its particles: or, it is refracted from the perpendicular with a subdivision of its particles, by which its qualities are altered: or, its passage is wholly interrupted, and it is either reflected or absorbed.

3. Since general attraction acts in bodies in the proportion of their masses and aggregation; since the more the affinity of aggregation is in any bodies destroyed, so much the more readily and powerfully do the chemical affinities of composition act upon them; since the extreme tenuity of light,

the subtilest of all visible substances, renders it infinitely sensible, equally to the general attractive force and to the chemical affinities of other bodies approaching it :—for these reasons, the same *general* and *chemical* affinities, which produce the other changes on material bodies, must appear sufficient to occasion the different phænomena which light presents in its various transitions.

4. The true primitive colour of light, when unmixed with other substances, when its particles exist together in their natural arrangement and aggregation, when the attractions of other bodies for it act not so as to scatter or decompose its parts, is *white*.

5. Its colours and its peculiar action on the optic nerve depend on the peculiar forms of its elementary particles, on their peculiar aggregation, on their mechanical and chemical qualities, on the modes of its *diffusion*.

6. It *passes without alteration or deflexion through transparent bodies* in which the absolute vacancies are sufficiently numerous to receive its rays, in which the disposition of these vacancies is perpendicular to the direction of the rays, and in which the attractions of the body for the light are not so strong as to act on the rays with a dissolving or a decomposing force.

7. It is *refracted*, when the vacuities in the bodies on which it falls are not disposed throughout the transparent medium perpendicularly to the direction of the rays; and when the attractions, chemical and mechanical, of the parts of that medium are, in respect to light, so powerful as to occasion either a tendency to the carrying of the ray into new combination, which shall alter its aggregation, or even a partial fixation of it in some new compound.

8. It is *reflected* when it falls on surfaces which present no *rectilinear vacancies*, none but such as are *curves*, through which its rays cannot pass; and when, at the same time, those surfaces have no affinities for its particles of sufficient strength to attract these particles into their own composition. When these circumstances concur in their utmost power the reflexion is complete, and all the falling rays are returned from the surface; but the required cir-



cumstances seldom do so fully concur. In most cases of the reflexion of light, there is a portion of the falling light either transmitted or *absorbed*.

9. Light is *absorbed* in the bodies on which it falls, when their attractions fix it in new compounds with their parts; or when they present a medium in which it may be for a time suspended, though not carried into actual combination.

10. As the primitive colour of light depends, in a great measure, on the conformation and the relative arrangement of its particles, so the other colours which it assumes in refraction depend likewise on changes in the arrangement of those particles which vary the effect of their conformation. While the particles remain in the same arrangement the colour is the same; and their elementary conformation, and the law of movement to which they are subject, continue their illuminating effect to the eye till they are lost by extreme diffusion, by absorption, or perhaps by entire decomposition.

The permanent colours of the surfaces of bodies depend on the same laws as the colours of the rays of light. The conformation and the arrangement of the extreme elementary particles at the surfaces of all bodies determine, respectively, their different colours. *White* surfaces, for instance, have one conformation and arrangement of the minutest particles at their extremities: *blue* surfaces have another conformation and arrangement of the same particles: *red* surfaces another, &c. &c. These extreme particles of the surfaces of bodies do not constitute light, because they are not unconfined and in motion, have not its peculiar rectilinear elasticity, possess not its chemical affinities; but when light is thrown upon them, they become visible, each set of particles under a different colour, just as its conformation and arrangement are different. The extreme particles of *blue* surfaces have the same conformation and arrangement as the particles of light in a *blue* ray; and so of other colours.

XII. *Some Account of FREDERICK AUGUSTUS ESCHEN, who was swallowed up in a Fissure of the Snow in the Glacier of Buet\*.*

**F**REDERICK AUGUSTUS ESCHEN was born in 1777, at Eutin, in the circle of Lower Saxony, where his father enjoyed a public office, which gave him an opportunity of having frequent intercourse with the archbishop of Lubec, his sovereign, by whom he was highly esteemed. The prince stood godfather to this son, and invested him with the canonicate. Being the eldest of a numerous family, young Eschen was destined for the study of jurisprudence, with a view that he might be fitted for holding some public employment; but his father did not confine his education merely to those branches of knowledge necessary for that line. He was instructed in the sciences and the fine arts; and, being early familiarised with both, he applied to them with unabated ardour, and made such a rapid progress as attracted the attention of some of the most celebrated men of Germany, among whom was Voss, who lived in the same village, and was an intimate friend of his father. Young Eschen was particularly noticed also by Count Stolberg, well known by his translation of Plato's Dialogues and his Travels through Swisserland and Italy.

At the age of twenty, young Eschen quitted his father's house, and repaired to the university of Jena, where he applied chiefly to philosophy and jurisprudence; the former of which he studied under Fichte, the disciple and rival of the celebrated Kant; and the latter under Professor Hufeland, a man as much distinguished by his private virtues as by his talents and learning. Natural history and philosophy occupied also some part of his attention; nor did he neglect poetry, to which he had a strong natural propensity. He translated several pieces from the Greek and Latin languages, which were almost as familiar to him as his own, and to which he had added almost all those of Europe. He pub-

\* See our last Number.



lished also various specimens of original poetry, many of which, of the Idyl kind, met with the approbation of Schiller and Goethe, the former of whom inserted one of them in the Almanac of the Muses, of which he is the editor. Another of them, entitled *Die Lehre der Bescheidenheit*, The Lesson of Modesty, deserves to be known on account of the ingenious manner in which the author treats a trifling subject little susceptible of poetical ornaments. Eschen wrote also several essays and dissertations on different points of antient literature, which appeared in some of the German journals, and which procured him an acquaintance with several men of eminence in the republic of letters. It is no little recommendation of Eschen's character, than we can mention among these Schlegel and Humboldt.

In the spring of the year 1798, Eschen undertook a journey to Swisserland in order to superintend the education of a young man, chiefly with a view that he might improve his own; and for that purpose settled at Berne, where one of his countrymen had been before engaged in the same occupation. Others soon followed him; and some young inhabitants of Berne, who had been the companions of his studies at the university, joined themselves to the circle of his friends. In the enjoyment of their company, with the charms of friendship and study, though amidst the storms of the revolution, he saw two years glide away in peace, and distinguished by labours which might have done honour to a longer life. His translation of the Odes of Horace had just appeared a little before the fatal event which snatched him from letters and from his friends. This production may be reproached with containing too many Latinisms and bold expressions little suited to the genius of the German language; but with all these faults it will still remain a classic work worthy of being ranked among the real treasures of German literature, and which ought to have inspired the greatest hopes of a young man, who, at twenty-three years of age, could venture to undertake a task so difficult.

The unfortunate catastrophe which terminated the life of this interesting young man has been already detailed in this journal.

journal. We shall therefore only observe that, about the end of July, this year, Eschen undertook, in company with M. Theodore Ziemssen his friend and countryman, a tour to the borders of the lake of Geneva and the valley of Chamouni. They ascended together the Buet, a high mountain behind the village of Servoz, celebrated by the experiments made there by De Luc and Saussure, and which commands a view of the country round Mount-Blanc. They were just on the point of reaching the summit, and nothing seemed to announce that any danger was to be apprehended; Eschen was walking forwards in high spirits before his friend and their guide, when, all of a sudden, his two companions lost sight of him. A thin crust of snow, which covered a deep fissure, had given way under his feet, and he fell into the abyss; where he perished as already related.

XIII. *On the Discovery of that Salt known under the Name of Seignette's Salt (Tartrate of Soda).* By Professor BECKMANN.

THIS neutral salt, which consists of the mineral alkali (soda) and the acid of wine-stone (tartareous acid), was prepared and made known by a Frenchman named Peter Seignette, towards the end of the last century. The confidence with which the inventor recommended it, and the care he took to conceal the method of making it, had, as is usual, such an effect, that it was employed in preference to many other medicines, long known; which had been equally serviceable; and by these means he was enabled, without much trouble, to acquire a fortune. It must, however, be allowed that he was a skilful chemist, who, by his writings and the invention of various other medicines, had obtained considerable reputation as a physician and naturalist. He was established as an apothecary at Rochelle; published papers on various natural objects which he had observed in his neighbourhood, in the Memoirs of the Academy of Sciences at Paris, as well as in other works, and died on the 11th of March



March 1719\*. He recommended this salt, which enriched him and rendered his name famous; in some small treatises printed, in particular, about the year 1672. He called it sometimes *alkaline salt*, sometimes *sal polychrest*, and sometimes *Rochelle salt*. After his death, his son continued to prepare and to vend it with the greatest success.

Manufacturers and mechanics have been often reproached with the jealousy which they entertain of literary men; but, in my opinion, the latter are the cause of it. It must indeed be confessed, though humiliating for human knowledge, that the most useful discoveries have at first presented themselves to the former, while engaged in the various operations which their employments require; but their merit consists principally in remarking and following phænomena till they produce from them something useful. If they are so fortunate as to succeed, they keep their discoveries secret in order that they may enjoy a monopoly of them; but no sooner has the man of letters heard of a new discovery than he wishes to have a share in the honour of making it known; and his zeal in this respect is proportioned to its importance and the care with which it is concealed; because, in general, he can gain only by rendering it public. The man of letters, however, has a great advantage over the mechanic or manufacturer, as his exertions never fail to be approved; because, by endeavouring to diffuse an important benefit, he appears in the character of a patriot, a friend to mankind, and a citizen of the world; and may thus place the merit of the mechanic or manufacturer in a disadvantageous point of view. This opposition of private interest proves of great utility to the whole society of which both parties are members. When the mechanic or manufacturer makes discoveries, they are communicated to the public by the man of letters; who, by these means, renders them useful; prevents their being hurtful by a monopoly; secures them from dying with the discoverer; and, by investigating the principles on which they

\* Some of Seignette's papers are printed in *Bibliothèque Historique de la France par Ferret de Fontette*, Paris 1778, 5 vols. fol. such as a paper taken from *Memoires de l'Acad.* 1707, p. 115; and also in *Histoire de la Rochelle*, par M. Arcère, Vol. II. p. 424.

depend,

depend, makes their benefit to mankind more certain, and shows how they may be applied in various cases of which the artist or manufacturer never had an idea\*. If, by this conduct, he lessen the merit of one, he on the other hand points out employment to many; and gives rise to establishments in which thousands participate, and by which they acquire riches.

Thus Seignette discovered sal polychrest while he was engaged in making soluble tartar (tartrite of potash), and, according to the old opinion, imagining that both the fixed alkalis were the same, used soda instead of the alkali of tartar (potash). By these means he procured, not without surprise, a salt different from the common soluble tartar, which he wished to prepare, and from the other well known salt also. He was induced therefore, to examine it; and having found it to be a new laxative, he recommended it and became rich. The experiments of learned chemists discovered the component parts of this salt; the mode of preparing it was then made publicly known; and, by more accurate examination, the difference, before overlooked, between vegetable and mineral alkali† was determined: by which new light was thrown upon chemistry, and an important service rendered to a variety of arts.

Among those who contributed to bring this salt into repute was Nicholas Lemery, to whom Seignette sent a large quantity of it, which he distributed at Paris, though unacquainted with its component parts‡. Its composition was discovered at the same time, about the year 1731, by two French chemists, Boulduc and Geoffroi. The former published his observations in the Memoirs of the Academy of

\* Nam invenire præclare, enuntiare magnifice, interdum etiam barbari solent; disponere apte, figurare varie, nisi eruditis, negatum est. *Plin. Epist. III. 13.*

† Professor Gmelin, in answer to the question, Who first remarked the difference between the vegetable and mineral alkalis, replied that, at any rate, it was first properly defined by Stahl. See G. E. *Stablii Fundamenta Chymicæ dogmaticæ et experimentalis.* Norimbergæ. 1746. 3 vol. 4to. III. p. 268 and 304.

‡ Lemery *Vollkommene Chymist.* Dresden und Leipzig, 1734. 2 vol. 8vo. I. p. 512.



Sciences \*; and the latter communicated his to Sir Hans Sloane, who caused them to be printed in the Philosophical Transactions †. I shall here observe that chulz ‡ has asserted falsely that Neumann made known the composition of Seignette's salt in his Treatise on Saltpetre; for Neumann's *sal polychrest* is essentially different; and he himself confesses § that he was not acquainted with the Rochelle salt. After the above period, the nature and properties of the mineral alkali were examined with more accuracy by Grosse, Duhamel, Brand (a Swede), and several others ||.

XIV. *On the Proportions of Charcoal, or Oxyd of Carbon, contained in certain Kinds of Wood and in Pit-Coal; and on a Carburet of Sulphur newly discovered. By M. PROUST ¶.*

**G**REEN oak yields of charcoal 20 *per cent.*; wild ash 17; willow 17; white ash 17; pine 20; heart of oak 19; black ash 25; guaiacum 24 *per cent.*: but all good pit-coals afford 70, 75, or 80 *per cent.* of carbonaceous matter; and there are some kinds which exhibit no signs of containing hydrogen; and which burn without either flame or smoke.

This abundance of carbonaceous matter yielded by pit-coals does not depend on their containing a larger proportion of earth; for good pit-coal yields as small a proportion of ashes as dried pine-wood. The pit-coal of Asturia and Andalusia yields only 2 or 3 *per cent.* of ashes; that of Estremadura not more than 6 or 7 *per cent.*

Besides the known products obtained from the distillation

\* *Memoires de l'Academ. des Sciences, Année 1731, p. 124.*

† No. CCCCXXXVI. p. 37.

‡ *Chemischen Versuchen; Halle, 1745. 8vo. p. 50.*

§ Neumann's *Chymie nach Kessels aufgabe. I. 3. p. 160.*

|| An account of the principal writings on Seignette's salt may be found in *Weigel's Chymie*, Griefswald, 1777, 2 vol. 8vo. II. p. 225. See also *Georgii Ludov. Enckelmann Diss. de Sale Alkali de Seignette, ejusque Natura et Usu. Argentorati, 1756. 4to.*

¶ From the *Journal de Physique*.

of pit-coals, I have reason to believe that they contain a small portion of succinic acid. The collected products from 30 *lb.* of pit-coal afforded me about one dram of a particular salt, which, by its smell, indicated the presence of that acid.

But what more particularly arrested my attention in the analyses of pit-coal was a very peculiar combination of some of the carbonaceous matter with a portion of sulphur, and where pyrites was not present. A coal in the vicinity of Almaden, in Estremadura, which contains no pyrites, yields very white ashes, and from which no sulphur can be obtained by distillation, exhibits, however, as it passes from incandescence to incineration, proofs of containing sulphur, which, becoming oxydated along with the coal, makes it sensible to the smell. A peculiar carburet of sulphur therefore exists in this coal; but combustion is necessary to its decomposition. Exposed to the greatest heat, this coal neither discolours silver, nor affects iron in the forge.

This singular phenomenon, happily perhaps for the arts, is one of the causes which so beneficially retard or prolong the combustion of pit-coal. Phosphorus combined with charcoal is much less combustible than when alone: neither the contact of atmospheric air, nor even the combined action of alkalis and water, is sufficient to separate phosphorus from its combination with carbon. From the difficulty with which the carbonaceous portion of animal substances is burnt, the same inference may be drawn respecting sulphur; for sulphur is known to exist in a considerable proportion in animal substances. Wool, unquestionably, contains much sulphur, but none passes over when that matter is distilled. Where then can it remain, if not in union with the carbonaceous residuum? That residuum should be examined with a view to this point. One thing is certain, that soap of wool, prepared in a silver vessel, deeply sulphurates its interior surface.



XV. *Letter from A. M. VASALLI-EANDI to J. BUVINA, Professor of Medicine in the University of Turin, on Animal Electricity\*.*

THE electric phænomenon which you observed in my electrometer, placed on the back of a diseased animal at the moment of its being attacked by a fit of shivering, appears to me to be a necessary consequence of the general theory of electricity, and of the modifications which it experiences in the animal economy. The following is the manner in which, I think, I have proved in my letter on the origin of animal electricity, that man, in the state of health, like all other animals, has parts positively electric, while others are negatively so.

It appears that, in the animal, the negative part, that of the excretions, is not so strong as the positive part, that of the blood. But if the change of the animal economy destroys the natural boundaries of electricity in the body, on account of the tendency of the latter to bring itself into equilibrium, it ought to escape and to manifest itself exactly at the moments when the boundaries are destroyed†; that is to say, when the virus changes the internal parts, which is proved by the fits of shivering: fear and other violent passions, as they change the animal economy, ought also to produce the same effect. Thus you have seen the strings of my electrometer, placed on the back of the animal, separate from each other, either during the fits of shivering occasioned by a contagious disease, or during those produced by fear; you see that the same theory explains also the want of electricity which you observed in diseased cats. I am persuaded that this want will in no case exist till the disease has continued several days, and the animal economy has become deranged. When I concluded my electric experiments on water and ice‡, I repeated them on several liquids, animals,

\* From the *Journal de Physique*, Pluviose, an. 8.

† *Journal de Physique*, Messidor, an. 7.

‡ *Memorie della Societa Italiana*, Vol. III.

and vegetables, as well as on different preparations of water. Urine and animal humours exhibited to me the greatest degree of electric difference. You see then that my opinion is supported by facts. As I have found, however, that the blood of those labouring under an intermittent fever is still positively electric\*, it might be useful to ascertain in what diseases, and at what degree of those diseases, it loses its electricity. Might not the electrometer be employed to distinguish desperate diseases, and be used, if I may be allowed the expression, as a vitalitometer? But many experiments are still wanting before we can reach that point of perfection in the science of electricity. The discovery of electricity in the torpedo seems astonishing. That of Cotugno, who received an electric shock from a mouse which he was dissecting; and that of Tonso, who had one from a cat; and my electric experiments on rats seem conclusive. But the immensity of nature still presents matter for new researches; and at present, since I have found the electricity of the blood and that of the excretions contrary, I see how much remains to be done to reduce to their just value the opinions of Galdini, Bertholon, Tressan, and Carlieu, on animal electricity. You have pursued the best route, which is to interrogate nature by experiments. Continue to do so, and you will enjoy the satisfaction of having enlarged the boundaries of science.

XVI. *Of Chemical and Mineralogical Nomenclature.* By  
 RICHARD KIRWAN, LL.D. F.R.S. and P.R.I.A.†

THE names given to the different substances known in common life, whether occurring in nature or produced by art, are coeval with languages themselves; and whether all were merely conventional, or some of them grounded on some relation to the thing signified, is now of little importance to inquire, as from long habit both are equally immediately referred to the thing signified, without any reflection on the

\* *Journal de l'Esquisse*, Germinal, an. 7.

† From the *Transactions of the Royal Irish Academy* for 1800.



original reason of their imposition. Thus, though the name *barometer* was originally imposed on the instrument so called, because the terms that compose this name, *baros* and *metron*, denote its use in measuring the weight of the atmosphere; yet the name is just as well understood by those who are totally unacquainted with its etymology, as by those to whom this is perfectly known. The instrument itself, and not its use, is denoted by the name, and equally occurs to both; the workman who knows not its use, knows what the name immediately signifies as well as the philosopher who employs it, and as well as the German who calls it a *schwermesser*, whose primitive components also express its use; nor is it better understood by either than the name *screw* or *schraube*, which cannot be resolved into any primitive component terms. Who, on hearing the Latin name of a book, ever thinks of its primitive signification—the bark of a tree? Languages must have been invented long before either chemistry or mineralogy were in any degree cultivated. In both the analytic and synthetic branches of chemistry as well as mineralogy, many substances must have occurred, to which, in common language, no name was applied, yet the necessity of denoting them by some name was urgent. Here, then, a difficulty occurred, which some sought to surmount by a name *arbitrarily* imposed, others by names derived from some real or fanciful relation of the given substance to some use, object, person, or particular quality or circumstance. Thus, in chemistry, *alcohol* appears to be a name arbitrarily imposed on highly rectified spirit of wine or very subtile powder. *Mercurius vitæ* denotes an antimonial preparation of great efficacy, as *kermes mineral* does one that resembles that substance in colour; *Glauber's salt*, a particular substance first formed by Glauber; *Epsom*, a salt first discovered in the springs near that town, &c. So in mineralogy, *quartz* seems to be a name arbitrarily imposed; and *spath*, a name originating from the resemblance of the integrant *lamellæ* to a blade; and *selenite*, from some fictitious resemblance to the moon. The etymology of these names was, however, soon forgotten or not attended to, and consequently, such of them

\* Some derive it from the Arabic *labala* exaruit.

as consisted of only one word, not evidently denoting some false relation, might without inconvenience be admitted; but in process of time descriptions were admitted instead of names, and these often false or absurd, as *sal mirabile*, *sal secretum*, *arcanum duplicatum*, *arcanum corallinum*, &c. The inconvenience and evident impropriety of many of these designations at last attracted the attention of those eminent philosophic chemists, Bergman and Morveau. Morveau, so early as the year 1782, published an excellent memoir on this subject in Rozier's journal, and his sentiments met with the entire approbation of Bergman. The new denominations he sought to introduce, soon however gave way to the more exquisitely devised systematic appellations grounded on the discoveries of Lavoisier, and the total elimination of the phlogistic element formerly admitted. These, in the formation of which Morveau also, in concurrence with a few of the most eminent Parisian chemists, bore a considerable part, were since admitted and recognised by most European chemists, and particularly in England. The exceptions that appeared to me reasonable to the general rules laid down by this highly respectable association, or to some of the terms they introduced, I thought of too little consequence to mention, knowing that the few antient denominations I retained, and the still fewer new ones I introduced, were perfectly intelligible; nor should I attempt at present to vindicate them, had I not perceived they attracted the censure of many on whose esteem I set the highest value. Thus circumstanced, I feel myself justified in examining the general propriety of those rules and assumed principles from which I thought proper to deviate, and of the denominations which I reject.

The first principle laid down by M. Morveau is, "that phrases are not a name; that substances and chemical products should be denoted by names fit to indicate them on every occasion, without having recourse to circumstances: p. 373. To this principle I give my entire assent.

Another rule laid down by M. Morveau is, "that in choosing denominations we should prefer those which have their roots in the dead languages more generally known, in order that the sense should suggest the name, and the name  
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the sense." It is for this reason, combined with the first, that I prefer single names, already understood, and well known by all those that have attained any knowledge of chemistry, to new names derived from the Greek or new unknown barbarous Latin names. Hence I prefer the term *bepar*, denoting in all chemical authors a combination of sulphur to every basis except a metallic, to the barbarous unknown Latin term *sulphuret*; and, with respect to metals, I prefer the well known terms *pyrites* and *galena*, to the newly-devised *sulphuret of iron*, or *copper*, or *lead*; the former has the advantage of denoting the particular specific combinations of sulphur with iron, copper, arsenic, or cobalt, and of excluding not only other sulphurated metals that do not give fire with steel, as the vitreous silver ore, sulphurated antimony, or lead, cinnabar, blende, sulphurated bismuth, &c.; but also such compounds even of copper and sulphur as do not give fire with steel, as the vitreous copper ore and the gray copper ore, and the purple. On these, and many other considerations which will presently be mentioned, I hope the ingenious Mr. Musket will recall his wish that I had modelled my Nomenclature on the principles of the French school. Phil. Mag. II. p. 156. C. Faujas, though of that school, remarks that these usual names (when otherwise faultless) cannot, without great inconvenience \*, be changed for others either erudite or barbarous. Journal des Mines, XXXV. p. 894. Nor is his opinion in the least invalidated by the note of C. Coque-

\* This puts me in mind of some Latin purists who change known modern names into what they esteem purer Latin ones. Thus, instead of *cancellarius*, they say *præfectus juris*; and for which Lipsius Not. ad Lib. I. Politicorum cap. 9. justly censures the historian Paulus Æmilius. "Vetustatis etiam nescio quid affectat in nominibus hominum, locorum, urbium immutandis et in veterem formam redigendis, sæpe erudite, interdum vane, sed, ut ego judico, semper indecore, quorsum *chartierius*, Gallorum *cancellarius*, *quadrigarius* mihi sit? et ille ipse *cancellarius* appellatur *præfectus juris*? et ubique Rex *Tarraconensis*, qui nobis et majoribus fuit Arragoniæ? talia infinita sunt *audacter* et *ambitiose* innovata et cum fraude pariter ac *cruce* lectoris." The Greek and Roman historians were frequently guilty of the same fault, which occasions at this day much perplexity.

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bert, who asserts that the new are more exact, sonorous, and significative; for any term is exact and significative when it denotes, without obscurity or ambiguity, the thing signified; and as to *sound*, it may deserve attention in poetry, but surely, if not exceedingly uncouth, it deserves none in science. He acknowledges that these alterations may cause some embarrassment to the present race of men, but thinks that succeeding generations will bless those that introduced them. I think on the contrary that they will curse them, for obliging them to learn both the new and the old denominations under the penalty of not understanding Stahl, Henckel, Margraf, Lémery, Geoffroy, Duhamel, Macquer, Bergman, Scheele and many others of the highest merit. Can any one be so arrogant as to pretend that these immortal authors can become unintelligible without prejudice to the science? Is it possible that musicians should judge more shrewdly than those who profess to be philosophers? Yet these have rejected all the modern alterations of notation that have been proposed to them, though attended with some advantages, from the single consideration that, if new modes were adopted, the inimitable compositions of the last and present age would either soon become unintelligible, or the arduous task of learning both methods of notation would be imposed on all succeeding generations. I am not however for the total exclusion of the term *sulphuret*; let it be employed to denote the composition of sulphur with any basis *in general*, whether alkaline, earthy, metallic, oleaginous, spirituous or carbonaceous; in this most extensive sense the old nomenclature supplied no term, and yet some name was wanting; in this sense therefore it may be retained.

Another general maxim advanced by M. Morveau is, that “the denomination of a chemical compound is neither clear nor exact except it expresses by names conformable to their nature the ingredients that enter into that compound.” This maxim, unhappily too easily adopted by the French school, tends to the subversion of the received language of all sciences, and even of common life. By this rule we are to banish the name *water*, and instead of it substitute its component ingredients



ingredients *hydrogenated oxygen* or *oxygenated hydrogen*\*; and instead of *ice* we are to say *decaloricated hydrogenated oxygen*, and for *steam*, *caloricated hydrogenated oxygen*. Instead of common *soap* we are to say *oleaginated soda*, and for *glass*, *solicited alkali*, &c.

The French chemists it is true retain the name *soap*, but in contradiction with their own principles; for they surely cannot in this word discover the radicals of its composition. Nay, Lavoisier retains the old word *nitre* and *salt petre* conjointly with that of *nitrat of potash*, (see his *Treatise of Chemistry*, vol. I. p. 79 and 232, French) and yet they disapprove of my retaining a few of the antient denominations that were as well known to all chemists as the names *soap* or *glass* in the language of common life, such as *Glauber*, *Epsom*, and *Sylvian* (rejecting only the term *salt* usually tacked to them; but evidently superfluous); also *selenite*, *gypsum*, *borax* and *alum*—these I retain for two reasons; first, because they express their respective objects by a *single* name, which appears to me a considerable advantage, and secondly, because those names continually occur in all treatises of chemistry published before the year 1790, and in many since, and consequently must be known by all who wish to understand them.

Morveau and the French school in general reject the names of inventors, “as having no conformity either generic or individual with things:” and for the same reason they should reject the names *Alexandria* and *Constantinople* derived from the founders; and in general, by the same rule, all names of places should be changed for such as would express their situation. Is it not therefore evident, when the signification of names is already fixed and generally known, that they should be retained†, the inconvenience of rejecting them being far superior to any advantages proposed by the change? Yet, strange to tell, they object to a few new names which I introduced in mineralogy, though exactly founded on their own principles, and not attended with any inconve-

\* Fourcroy expressly says water may be called *oxyd of hydrogen*. See St. John's *Method of Nomenclature*, p. 64.

† 19 Roz. p. 374.

nience, as they denote objects not known by any denomination previously assigned to them, and even possessing peculiar properties of great importance. Thus they disapprove\* of the name *muricalcite*, a name which I thought proper to bestow on such limestones as contain a notable proportion of magnesia; and *baryto-calcyte* on such as contain a notable proportion of barytes; these stones having been distinguished by no name, and these new names being in conformity with the principles of the French school derived from and grounded on their component ingredients.

Mr. Tennant has since shown, in an important paper in the Philosophical Transactions for 1799, the evident necessity of distinguishing these *muricalcites* from common limestones, with which they have always been confounded; and, from the deleterious properties which barytic lime water possesses, it is evident that the limestones that contain any proportion of barytic earth cannot without danger (sometimes to human life) be confounded with common limestones. The stone which the Germans without any inconvenience called *apatite*, approaching in our language too nearly the word *appetite*, I have called *phosphorite* to mark its composition; but as the phosphoric acid has also been detected, or at least suspected, in composition with argil, to distinguish this compound from the former I call it *phospholite*, a name better suited to it than that of *Valentia garnet*, by which it was formerly denoted—a change attended with no inconvenience, as the stone itself was known in no part of Europe but Spain. To these denominations they object their *monotonous terminations*; but these terminations are best suited to our language; they do not recollect that the terminations in *at*, as *nitrats*, *sulphats*, *muriats*, *carbonats*, *phosphats*, *oxalats*, &c. &c. are equally monotonous in theirs. They think, or rather magisterially decide, that minerals should be denoted by the same name as similar chemical compounds; a rule that might be admitted if such minerals were not previously generally known by other single appellations, and if their composition was perfectly similar to analogous chemical compounds. But in the first place several minerals are already

\* Ann. Chym. XXXIII. p. 103.



generally known by peculiar proper denominations; for instance, *cinnabar*, surely a more convenient name than the drawling new name *sulphurated red-oxyd of mercury*: and in the next place they should consider that chemical compounds being artificial productions may contain only those ingredients which their compound names import. But minerals whose principal composition is analogous, often contain other substances also, which the new chemical name would not express, and would thus lead to important mistakes. Thus, for instance, *phosphorite* is commonly contaminated with *silica*, aerated lime, muriated lime and iron, and sometimes with manganese and fluor; whereas the analogous chemical compound on which the name *phosphorated lime* is properly imposed, is free from such contaminations. Substances, therefore, so different, should certainly be distinguished by different names, or at least the word *natural* or *native* must be tacked to the name of the chemical compound; and though, with respect to fossil metallic chemical compounds, this inconvenience may often be avoided by the addition of the word *ore*, yet here also the antient name often expresses some other inherent property; thus the term *vitreous* added to silver ore denotes not only the composition, but also the easy fusibility of that particular ore.

[To be continued.]

## NEW PUBLICATION.

*New Observations concerning the Colours of thin transparent Bodies, showing those Phænomena to be Inflections of Light, &c.* Cadell and Davies, Strand, 1800.

THESE Observations are by the author of two treatises on the *Origin of the Diversity of Colours* in the Rays of Light, and on the *many-coloured Coronæ* occasionally appearing round the Sun and Moon; of which the leading principles have been stated in former Numbers of the *Philosophical Magazine*.

The object of the present treatise is to explain, in a manner consistent with the leading principle formerly advanced by the author, another important class of the phænomena of light and colours.

Sir Isaac Newton has represented the smallest parts of all natural bodies as transparent. He supposes those parts to be placed in a medium, inferior in refractive power to themselves. Those bodies are in his estimation transparent, which consist uniformly of the smallest parts and the smallest intermediate spaces; those are black, which have their integrant parts and the intervening spaces of a size somewhat greater: such as are variously coloured consist of parts larger than those of either transparent or black bodies. The rays of light are, in his judgment, disposed, by a natural indestructible quality, to be alternately transmitted through opposing media, and reflected from them. By this quality, the transmissions of light, its refractions and its reflections, appeared to him to be in all cases regulated.

The author of this treatise, on the contrary, denies that the minutest integrant particles of bodies are known to be transparent; that there exist within bodies any such refractive media surrounding their parts as Newton has imagined; that refraction and reflection depend in any measure on the arrangement and conformation of the parts of bodies, which Newton supposes; or that light is, by a mysterious natural quality, disposed, by turns, to easy transmission and to easy reflection.

He maintains, that all the phænomena which Newton has thus erroneously explained, are only so many different cases of the inflection of the particles of light by the attractions of bodies approaching them. According to him, as it should seem, light is never, in any circumstances, made to exhibit a diversity of colours in its rays, otherwise than by the points or edges of approaching bodies exerting on it an attractive force sufficient to destroy the natural consistency of the whole, or at least a part, of the white ray. Refraction and reflection depend upon no peculiar qualities in light, but simply on the attraction exercised upon it, *according to the general laws*, by the other bodies with which, in various circumstances, it comes



comes in contact. He has made several ingenious experiments, which are in this treatise related in detail, and of which the common result seems forcibly to suggest the conclusion which he thinks that he has established. Much of the treatise is employed in pointing out the errors as to fact, and the fallacies of induction, which appear in the explanations by Sir Isaac Newton, concerning this part of the science of optics.

To say the truth, we find the facts and reasonings in this essay somewhat less satisfactory than those in its author's two former essays. It is probably the true principle which he has discovered: but he does not, in this instance, see it clearly; he only gropes at it, as it were, in the dark. Perhaps, however, not the author, but the dullness or impatience of the reviewer, may here be chiefly in blame.

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## INTELLIGENCE,

AND

## MISCELLANEOUS ARTICLES.

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### LEARNED SOCIETIES.

#### ROYAL SOCIETY OF LONDON.

ON Thursday, Nov. 6, this learned and useful body held their first sitting since the long vacation, when the commencement of Dr. Herschel's Second Part of his Inquiry respecting Heat and Light was read.

On the 13th the Society occupied itself in reading the conclusion of Dr. Herschel's paper, which contains a detail of a number of most interesting experiments relative to the different quantities of heat and light transmitted through various substances and through different coloured glasses. From Dr. Herschel's experiments it appears that red glass allows most heat and least light to pass through. One experiment

is detailed, which, though he says it was coarsely conducted, leads him to conclude that the focus of heat falls at the distance of half an inch from that of light. One hundred and seventy experiments are recorded, from whence he concludes, that, as the laws of the motion of light and heat are essentially different, there is the greatest reason to conclude that they are not the same fluid.

The Croonian lecture, on the irritability of the nerves, by Everard Home, was begun reading the same evening, and concluded at the following meeting on the 20th. In this paper Mr. Home denies the hypothesis of a peculiar nervous fluid.

#### SOCIETY OF ANTIQUARIES.

Thursday, Nov. 6, the Society met for the first time since the summer vacation; on which evening, and on the 13th and 20th, several very curious fragments of the paintings from the walls of St. Stephen's chapel, representing various subjects from the books of Job and Tobit, were exhibited; and a very curious memoir on the subject, by their learned Secretary, was read. The President addressed the Society, and informed them that the Society had appointed a committee for the purpose of superintending the execution of drawings which were making of all the curious remains that have been discovered during the alterations that were carrying on at St. Stephen's chapel, and which of late had excited such general curiosity among all classes of people.

#### SOCIETY OF NATURAL HISTORY AT PARIS.

C. Haüy lately read before the Society a note on the crystallisations of iron ore. The intended publication of a treatise on mineralogy by this author having induced him to revise, with great care, what he had written on crystallisation, he found that he had rectified only in part the incorrectness into which he had fallen in regard to the crystalline forms of iron ore.

He had already announced, in an extract from his treatise, that the crystals of volcanic iron were not segments of the regular octaedron, as believed, and that the difference between their angles and those of these segments was more than



12 degrees. He has since found that they have for their primitive form a rhomboid somewhat acute, in which the angle of the summit is about 87 degrees.

But he thought also, with all other naturalists, that the crystals of the iron of the island of Elba were derived from the cubic form; and he had referred to that form that of the crystals of Framont in dodecaedra composed of two right pyramids incomplete. He had always been struck, however, with a kind of singularity presented here by the cubic form which performed the function of the rhomboid; that is to say, that it was necessary to suppose an axis passing through the two opposite solid angles, which were to be considered as the summits, and the laws of the decrement which took place around the summits were different from those which related to the lateral angles.

He was still more surprised, when, having lately tried to apply theory to a variety of the iron of Framont, which he had not before examined, he observed that it was necessary to suppose it to result from a decrement by twenty rows on the inferior angles of the primitive cube, in order to have results corresponding with observation.

This law, though perfectly admissible, deviated so much from the usual laws, that it inspired the author with suspicions respecting the cubic form itself, and, by the help of goniometry, he measured, for the first time, on the crystals of the island of Elba, the mutual incidence of the primitive faces; whereas he had before confined himself to measuring that of the faces produced by the decrements, either among themselves or on the primitive faces, having no idea that there could be any uncertainty in regard to a form which presented in so sensible a manner the appearance of a cube; and the more so as the facets by which it was modified prevented the difference from being observed. He perceived that this form was a real rhomboid similar to that of volcanic iron; and in that case, this law, which had appeared so singular on the hypothesis of a cube, gave place to a simple law, and every thing, as we may say, was reduced into proper order.

In regard to the varieties of the iron of the island of Elba,  
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he found no change to be made in the old laws; because the secondary incidences which he had determined in the supposition of a cube, differed only half a degree from those resulting from the rhomboidal form. This, says the author, is one of those cases where a quantity very sensible of itself is diminished by passing into certain results which depend on it.

It arises from these researches, that all the iron ores which retain a metallic appearance may be reduced to two kinds, very distinct from each other; one of which contains substances that crystallise in regular octaedra, such as the iron of Corsica; and the other, those having for their primitive form a rhomboid somewhat acute, as the iron of the island of Elba, that of Framont, and that of volcanoes. The first will continue to be distinguished by the name of *oxidulated iron*, and the second will be called *oligist iron*; that is to say, little abundant in iron in the metallic state. It is here seen, that a greater quantity of oxygen imprints on the primitive form a character entirely peculiar, by making the regular octaedron pass to the rhomboid; and this seems to indicate two very distinct points of equilibrium, which chemistry no doubt will determine when it has carried the analysis of iron ore to a degree of correctness, suited to that perfection which this science has already attained.

#### PHILOMATIC SOCIETY.

C. De Saussure, the son, read lately before this Society a memoir on the influence which the soil has on certain constituent parts of vegetables. It was formerly believed that the soil had no influence on vegetables, but in proportion to the faculty it possessed of retaining a greater or less quantity of moisture; and it was to this cause alone that the difference between the abundance and size of vegetables growing in calcareous soil, and the same qualities in those found in granitic soil, was ascribed. But Saussure having remarked that the animals which live in the calcareous lands, that produce these vegetables, were larger, fatter, and gave a greater quantity of milk, richer in butyraceous and caseous parts, than those in granitic districts, conceived an  
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idea, that there must exist between these vegetables a difference still more important, and which ought to depend more on the nature of the soil. Saussure therefore has made a series of experiments, with a view to ascertain the difference which the nature of different soils produces in the composition of the vegetables which grow in them. That these experiments might be conclusive, it was necessary to make them very much comparative, that is to say, to take the same quantity of the same vegetables, of the same age, growing under similar circumstances or in the same exposure, at a distance from the course of streams, and beyond the reach of cattle.

It was necessary also to repeat the same experiments a great number of times in order to obtain mean results, and to approach the truth by multiplying probabilities. This C. de Saussure did. He began by analysing the stone that composed the mountains the plants of which he examined. He then proceeded by the usual chemical means, which he describes in his memoir, to analyse the vegetables, with a view to ascertain the respective quantities of natural water, charcoal, earths, and salts which they contained. That he might obtain more general results he operated on different kinds of plants, viz. the *pinus abies*; *pinus larix*; *rhododendron ferrugineum*; *vaccinium myrtillus*; *juniperus communis*. All those vegetables which belonged to granitic districts contained more water than those of calcareous countries. The smallest quantity of the extreme difference was as 57 to 58, and the greatest as 52 to 59. These differences cannot be ascribed to the quantities of water which might be retained in the mould of the granitic soil and that of the calcareous, for they are the inverse of those presented by the vegetables of these two soils. De Saussure concludes with Duhamel, that the wood of calcareous countries is preferable, in point of solidity, to that of granitic countries.

Proceeding then to a comparison of the quantities of charcoal contained in vegetables, Saussure remarks how difficult it is to estimate with precision the absolute quantities of charcoal; the relative proportions of that principle can at most be known; and he found that it is more abundant in the

calcareous vegetables; so that it seems to supply the place of that quantity of water which they had less than the granitic vegetables.

Granitic vegetables being more aqueous, ought, according to the observations of Duhamel, to have a softer texture, and consequently to contain more ashes. The incineration of granitic and calcareous vegetables gave differences too difficult to be appretiated; but, however small they might be, they appeared to confirm this observation.

The ashes furnished by incineration having been carefully analysed, Saussure found in those of calcareous vegetables a greater quantity of that earth, and much more *filex* in the granitic; so that the ashes of the calcareous rhododendron in 100 parts contained 57 parts of the carbonat of lime and 5 parts of *filex*; while those of the granitic rhododendron contained 30 parts of carbonat of lime and 14 parts of *filex*. This great difference is one of the most convincing proofs of the influence of the soil on vegetation. As the lime-stone of mount la Salle, where Saussure collected the vegetables the ashes of which he analysed, contains *filex*, this chemist was desirous of knowing whether vegetables collected on soil entirely free from that earth contained any of it. He therefore analysed the ashes of some plants which had grown on lime-stone totally free from *filex*, in the mountain of Reculey-de-Thoiry, in Jura, and found in one or two cases only a very small proportion of *filex*, while in the ashes of the vegetables of Breven he perceived much more calcareous matter than that mountain could furnish. Saussure thence deduces this geologic conclusion, that vegetables cover with calcareous matter mountains which have a base of *filex*, while the inverse of this does not take place.

In the last place, he made a comparative analysis of the mould, in which the plants of Breven and those of Reculey-de-Thoiry grew, in order to determine the relation which ought to exist between that mould, the soil, and the ashes of vegetables which grow in it, and had nearly the following results: The mould of Breven gave 60 of *filex*, 14 of alumine, 1.16 of lime, &c. That of Reculey-de-Thoiry,



15 of filex, 37 of alumine, 23 of carbonat of lime, &c. It must here be remarked, that neither the soil nor the plants of that mountain contained any appretiable portion of filex.

## FRENCH NATIONAL INSTITUTE.

C. Lacepede read a memoir on the ant-eater.

A memoir was read by C. Beauvois on the American fox, and the rabbit of the same country. A comparison of the skull of the European fox with that of the American fox, *canis virginianus* Gmel., as well as of the European rabbit with that of America, *lepus americanus* Gmel. has evidently shown that these two species have been improperly considered by Buffon and several other naturalists as mere varieties of the European species, and that Erxleben and Gmelin were authorised in making them distinct species.

Foxes, like the dog, bear, badger, and several other animals of the family of the *feræ*, have on the top of the head two salient lines, which proceed from the posterior angle of the orbit and proceed backwards. In the European fox these two lines unite at the suture of the frontal bone, where they form a ridge more or less salient according to the age of the individual. In that of America these lines are three times as large and as prominent; instead of uniting at the frontal bone, they separate from each other, and extend as far as the occipital ridge, where they unite.

The lower jaw in these two animals presents still other very sensible differences. Each branch, which in the European fox appears under the form of a well-rounded curve, is straight in the American fox, and forms, with the ascending branches, an angle of nearly  $145^{\circ}$ .

Differences equally striking are observed between the European and the American rabbits in the elevation and thickness of the orbicular apophysis. The American rabbit never burrows, like that of Europe, and always brings forth two young, like the hare. From all these characters it appears that the American rabbit is an intermediate species between our hare and our rabbit.

C. Decandolle communicated some experiments respecting the influence of light on certain kinds of vegetables. The

object which the author first proposed in his researches was to ascertain the influence which light has on the sleep of the leaves and flowers of plants. It appears to the author, that regular vicissitude of day and night in the ordinary course of things renders any researches in this respect exceedingly difficult; and he thinks that they might be rendered much easier if vegetables were exposed to an artificial light, continued or combined in various ways. For this purpose he placed six lamps in a dark cellar, and disposed them in such a manner that the illuminated plants had only about 66 or 67 degrees of heat, and were sheltered from the smoke. These six lamps gave a light equal to 54 tapers. The experiments which he made with this apparatus are as follow:

Mustard (the *myagrūm sativum*) and cresses, sown, reared, and made to grow, exposed to this artificial light, had a sensibly green colour; but their stalks were a little longer than those which grew in the open air.

The leaves of different plants put under water, and exposed to the light of these lamps, produced no oxygen gas for twenty-four hours; afterwards they putrefied, and formed a deleterious gas. This result is not astonishing, for six lamps are not equal to the light of the sun; and it is well known that in the shade no oxygen gas is disengaged.

Branches of the lime-tree, *jolanum lycopersicum*, immersed in water and exposed comparatively to the light of these lamps, an obscure heat of 69° Fah. and the open air during night, attracted much more water in the light than in obscurity. Branches of the oak attracted little water in the light, and a great deal when exposed to heat. Branches of the fir attracted very little in the light. It would appear that this element has a stronger action on vegetables that shed their leaves than on ever-greens.

The cessation of suction and of transpiration during the night is a real sleep common to all plants. This name, however, is given to a particular position which the leaves and flowers of certain plants assume in the night-time.

Linnaeus distinguishes the solar flowers into three classes; the *meteoric*, *tropical*, and *equinoctial*. C. Decandolle is of opinion that to these we ought to add the *ephemeral*, which



flower at a certain hour, and perish at a certain determined period, which never exceeds twenty-four hours. The marvel of Peru (*mirabilis jalapa*) when exposed to the light of the lamps for three days, continued to open in the evening, and to close together in the morning, nearly at their accustomed hour. The case was the same in total darkness; but, being exposed to the light of the lamps during the day, they first exhibited some irregularities, but on the second day they opened in the morning and closed up at night. The *convolvulus purpureus*, which unfolds itself in the openest air at ten in the evening, having been exposed to the light of the lamps, opened the first day at ten in the evening, and next day at six.

The rock-rose, broad-leaved tree-primrose, bind-weed, ficoïdes, marygold, and the diamond fig-marygold, exhibited a great number of variations mentioned by C. Decandolle, which it would be too tedious to detail here. We shall only add, that the Egyptian fig-marygold (*mesembryanthemum noctiflorum*), when exposed to light during the night-time, and to obscurity during the day, opened in the morning and closed themselves at night; and that the *mesembryanthemum splendens* and *tenuifolium*, exposed to a heat of 69°, opened their flowers in a very short time, while the same heat had no influence whatever on other plants.

Bonnet, who has endeavoured to explain the sleep of plants, supposes that the lower surface of the *foliole*, such as those of the false acacia for example, is susceptible of extending by moisture, and that its upper surface is susceptible of extending by dryness. But C. Decandolle observes that the moving cause seems to act on the insertion of the *foliole*, and not on the entire surface; that this explanation cannot be applied to the leaves the *foliole* of which inclines forwards or backwards; and that it would be necessary to admit that the *sophora* and the *guilandina*, which in the night throw down their leaves, are organised in an inverse manner to the false acacia; which is not confirmed by anatomy. The cause of the sleep of plants is therefore really unknown.

In none of the experiments was there any change made in the progress of the *oxalis stricta* and *incarnata*, but a power-

ful

ful influence was observed on the sleep of the sensitive plant. Several sensitive plants, exposed for three days to the continued light of the lamps, opened and closed each day two hours sooner than the day before; from which it appears that the continuance of light hastened and did not interrupt their movements. When exposed to the same light during the night, and to obscurity in the daytime, they exhibited a regular progress for nearly two days; after which they expanded in the evening and closed in the morning. Their movements were not deranged by total obscurity, but they seem to have been retarded by a heat of  $60^{\circ}$  to  $80^{\circ}$ . A heat of  $78^{\circ}$  rendered the plant sickly, and deprived it for two days of its sensibility when touched.

These facts, according to the author, can be explained only two ways. We may say that these periodical movements are peculiar to the fibres of plants, and that external circumstances are only the stimulants which excite or retard them; or that the periodical movements continue notwithstanding the change of external circumstances, only on account of the habit acquired by the fibres. This last explanation appears more probable than the other, because we are already acquainted with some facts which seem to indicate that plants are susceptible of habit. But, whatever may be the explanation we adopt, we must admit as the foundation of it the theory of irritability; that is to say, to acknowledge that vegetables are endowed with a peculiar kind of life or force; in consequence of which their fibres are not affected by external bodies, as unorganised beings would be from the mere laws of mechanics.

The English journals having announced some new experiments respecting the theory of Galvanism, a memoir on which had been presented to the Royal Society by Professor Volta, these experiments have been repeated by C. Robertson, and by a commission chosen by the Institute.



*MISCELLANEOUS ARTICLES.**ANTIQUITIES.*

The lovers of British antiquities in general, and of Gothic architecture in particular, will be pleased to hear that Mr. Lowry, of Titchfield-street, engraver, and Mr. Alexander, of Newman-street, well known as draughtsman to the late Chinese embassy, intend to publish a selection of picturesque and accurate Views (about the size and in the manner of Hearn and Byrne's *Antiquities*) of the crosses and conduits erected at different times in various parts of this island, many of which, from their present decayed state, and there being no accurate representations of them, must otherwise soon be irrecoverably lost. The drawings will be made by Mr. Alexander, and the plates wholly engraved by Mr. Lowry; and, from the known abilities of these artists, the public have a right to expect a work equal in execution to any thing of the kind hitherto published.

A prospectus, with a specimen, will, we understand, be soon laid before the public.

*AGRICULTURE.*

The following useful hints we copy from a Paris paper:

“ A Journal, which has a very extensive circulation, has lately announced secrets for curing the smut in wheat, preserving it from weevils, and for making bread from smutty wheat as good and salutary as that made from sound wheat. As a farmer, I take the liberty of giving my opinion respecting these discoveries.

“ I can certify with truth, that the method of Trianon, which consists in washing the seed in a solution of lime in water, has always preserved my wheat from smut since I have made use of it. In my opinion it is impossible to find a preventive less expensive. Besides, I do not employ it but for the fifth part of the wheat which I sow each year; that which arises from it is employed for seed the next year, and so on in succession.”

succession ; but I take great care not to dry the wheat prepared in this manner either in an oven or stove, lest the germ should be injured ; it must not even be dry when sown, because the lime would detach itself, and injure the sower.

“ In regard to weevils, one of my neighbours has entirely cleared his house of them by a very easy process. In the month of June, his granaries and barns being entirely empty, he caused a number of large ant-hills to be collected in bags, and dispersed them throughout the places infested by these insects. The ants immediately attacked the weevils and devoured them entirely, so that not one of them was to be seen. Since that period none of these destructive insects have appeared on his premises.

“ The third secret is very simple : it consists in washing the smutty wheat in pure water till it no longer becomes black ; it must then be immersed in boiling water and dried, so that it can be ground. By this process as good bread may be obtained as that made from wheat which is free from smut.”

#### CHEMISTRY.

Mr. Klaproth, of Berlin, having lately analysed *boney-stone*, has found that the alumine contained in it is united to an acid, the radical of which is the same as that of the vegetable acids, but with different proportions of the carbon and hydrogen.

The vegetable alkali has been found in different minerals. This new fact cannot therefore fail to prove interesting, and may lead to some useful facts respecting the connection between the vegetable and mineral kingdoms.



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THE  
PHILOSOPHICAL MAGAZINE.

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DECEMBER 1800.

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I. *A short View of the new Electric Experiments performed  
by Dr. VAN MARUM.*

THE experiments which form the subject of this article were performed with the large machine in the Teylerian museum at Haarlem, partly undertaken at the request of other philosophers, and described in his last work on electricity, entitled *Tweede Vervolg der Proefneemingen gedaan met Teeyley's El. Mach. Haarlem 1795; 4to.* It must here be observed that, since the last publication respecting these experiments, great improvements have been made in the large machine, and particularly in regard to the cushions. According to the old mode of construction, the cushions were pressed towards the glass plates by means of two screws, in consequence of which it was not possible to maintain an uniform pressure on both sides. At present, however, an uniformity of pressure is obtained by two steel springs, which are applied with hinges to the end of two iron plates, and are kept together by one single screw. As these springs exercise their pressure on the centre of gravity of the cushions, the pressure is uniformly the same in every part. Besides this improvement, the conductor is constructed in such a manner that, merely by turning, it can be employed sometimes for receiving positive and sometimes negative electricity; so that both these kinds of electricity can be communicated to the conductor by changing its position. The collector is no longer

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longer furnished with spikes, but is perfectly smooth and somewhat rounded, and has been lengthened from six to ten inches.

By the conductor alone experiments were made.

1. Respecting the effect of electricity on the pulse. Dr. Van Marum thought he had reason to conclude from his former experiments, that electricity does not increase the pulse. But, as some doubts arose on this subject, which were supported by a paper of Messrs. Von Troostwyk and Deiman, he thought it might be of some utility, in a subject of so much importance to medical electricity, to repeat the experiments according to the method of these philosophers. For this purpose eleven persons were selected, and the experiment was repeated four times on each, both with positive and negative electricity. These persons were placed in a room which was at such a distance from the machine that they could not hear the noise it made in turning; they were also insulated, and their pulse was felt when the machine was in motion as well as when it was at rest (which last circumstance was unknown to them), and the beats were told by a good observer by means of an excellent watch. In some single cases a few beats more were observed, but on the whole there was no particular increase of any consequence. In general, however, there was great irregularity in the pulse both during the time the persons were electrified, and during the time the machine was at rest.

2. Respecting the increase of insensible perspiration during the time the persons were electrified. Dr. Van Marum employed for this purpose a very sensible balance, one scale of which was insulated by means of a silk cord. On this scale he placed a boy, eight years of age, connected with the conductor, and brought the balance into equilibrium. He then examined the loss of weight sustained in half an hour before the boy was electrified, and found it amount to 280 grains; after which the machine was turned for half an hour, and the loss at the end of that time amounted to 295 grains. By a similar experiment on another occasion, the loss of weight before being electrified was 330, and after being exposed to electricity only 310. A girl of seven years lost, before being  
 8 electrified,



electrified, 180; and when electrified, 165 grains. A boy of eight years and a half lost unelectrified 430, and when electrified 290. Another of nine years, unelectrified 179, electrified 240. As the last boy was exceedingly quiet during the experiment, it was thought that the increase was the consequence of electricity; on this account he was several times subjected to the experiment, and the results were in the unelectrified state 550; in the electric 390, 330 and 270, 550 and 420. In most of the experiments it appeared that there was rather a decrease.

3. Respecting the irritability of the vessels of vegetables as the cause of the ascent and descent of sap.—The result of these experiments (*viz.* that from the cut stems of different kinds of euphorbia, and other plants of the like nature, when exposed to strong sparks, no more sap flows, as, by the irritability of the fibres being destroyed, the vessels are rendered incapable of contracting themselves) is already well known.

4. Respecting the existence of caloric in the electric matter.—Dr. Van Marum caused a conductor of very thin brass plate, five inches in diameter and eleven inches in length, to be constructed with a cavity in the middle, in which he placed the bulb of a very sensible thermometer, and suspended it by silk strings near the conductor of the large machine. Neither by positive nor negative electricity, however, did their appear the least sign of the thermometer rising. As charcoal is an excellent conductor, he introduced the bulb of the thermometer into a cavity made in a piece of that substance; but still there was no sign of heat. From this it appears to him, that the signs of heat exhibited by electricity may arise only from the great velocity with which the electric matter passes through bodies, and that the fusion or combustion of these bodies thence resulting may be occasioned by the friction thus produced. If a stream of electric matter be conveyed to the bulb of a thermometer, it immediately rises as Van Marum found, and often from 80 of Fahrenheit to 100 and more: but this experiment cannot be considered as a proof of the existence of caloric in the electric matter, as Cavendish found the electric current decomposes the atmospheric air, by which means some caloric may be dis-

engaged from it. To ascertain the truth of this conjecture, Van Marum introduced a thermometer into a receiver between two conductors, and, having exhausted part of the air, threw the electric stream on the bulb. The thermometer, however, rose higher than in the open air, or to 120 degrees. The air in the receiver had been rarefied to  $\frac{1}{60}$ . That he might proceed with still greater certainty, he performed the experiment in oxygen gas and azotic gas rarefied in the same degree; but in both cases the thermometer rose as much as before. Van Marum thought he should find another proof of the above opinion, by trying whether the electric matter was able to convert aqueous liquids into expansible fluids; for, as the elasticity of all such fluids is ascribed to their mixture with caloric, it seemed reasonable to conclude that caloric must exist wherever it is possible to form expansible fluids. Priestley converted vitriolic ether, by means of electric sparks, into inflammable gas, and obtained the like result from oil of turpentine, spirit of wine, and ammonia; but Dr. Van Marum, from these substances, obtained only very little gas, though his machine worked with much greater strength, and the small quantity which was obtained was again soon absorbed; and therefore he is of opinion that these gases were rather expelled from the above substances by the electricity, than prepared by it from their component parts. With ether and ammonia the quantity was a little larger; but, as these substances are exceedingly volatile, it could not be determined with certainty that the air was produced from the caloric of the electric matter; because several liquids acquire their liquid form merely from the pressure of the atmosphere. Dr. Van Marum conceived the idea of making the experiments in vacuo with other fluids, because in that case a small quantity of caloric might produce air. For this purpose he employed the Torricellian vacuum, and, having put platina wires into several barometric tubes  $\frac{1}{2}$  inch in diameter, fused the glass around them at a lamp; he then inverted the tubes, and filled them with quicksilver in such a manner that  $\frac{1}{5}$  inch of each remained empty. Into this empty space he introduced the fluids, through which the electric sparks were to be conveyed; then closed the aperture, and again inverted the

the



the tubes, that the fluid might ascend to the upper part of them. The exhausted space left by the descent of the mercury was some inches in length, which appeared to him to be the most advantageous. He then held the tubes in a vertical position in a vessel filled with mercury, and, placing a ball three inches in diameter on the wires at the top of the tubes, caused the sparks from the conductor to fall upon it. The quicksilver in the tube was at the same time connected with an insulated ball by means of a wire.

The first experiments were made with water, carefully purified by boiling and by the air-pump. When the sparks struck the water through the vacuum, a considerable quantity of air seemed immediately to be produced, so that in three minutes the mercury fell  $1\frac{1}{2}$  inch. During the next five minutes the mercury fell only  $\frac{1}{4}$  of an inch, and the production of air then totally ceased. After three days, the air which had been produced did not appear to be in the least lessened. By another experiment of the like kind such a quantity of air was produced that in four minutes the mercury fell 3 inches 4 lines; but next day the air was lessened, 1 inch 8 lines: the remainder retained its elasticity.

In an experiment with alcohol, such an abundant quantity of air was produced that at first the mercury fell  $\frac{1}{2}$  inch at each spark. The production of air, however, decreased in proportion to the falling of the mercury. Two experiments of the same kind gave more striking results, which in other respects were similar to the former.

In an experiment with caustic ammonia, a column of air of 21 inches was produced in five minutes. Carbonat of ammonia gave a column of 18 inches, and camphor one of  $6\frac{1}{2}$  inches in the same time. The air produced by alcohol, when tried by a test, was found to be pure inflammable air: that from camphor was found to be nearly as free from mixture: that, however, from both kinds of ammonia consisted of inflammable air mixed with azot. From this it appears that the electricity had separated from each other the two component parts of the ammonia, hydrogen and azot. Dr. Van Marum imagined that the air produced from the water would consist also of oxygen and hydrogen, and on that ac-  
count

count tried to inflame it, but without success: he condensed the air he obtained till it almost equalled in that respect atmospheric air, by immersing the barometric tube in a wider one filled with quicksilver; but inflammation did not take place until a little oxygen gas or atmospheric air was introduced into it. Hence there was reason to conclude that inflammable air only was obtained from the water; and Dr. Van Marum acknowledges that it is difficult to explain what came of the oxygen, the other component part of the water. Why, says he, did it not form itself into oxygen gas with the caloric produced from the electric matter? But it is possible, adds he, that this last formation may be much more difficult than the former; and, as it appeared from former experiments, that the electric spark decomposed oxygen gas, its oxygen may have passed into the mercury, and the caloric may have escaped: no signs of oxydation, however, were observed on the mercury.

All the kinds of air produced in this manner, that from water excepted, fully retained their elasticity; for even at the end of a year no lessening of them was observed in the tubes, though kept under a pressure equal to that of the atmosphere. Electricity, therefore, had effected in these experiments what had been formerly ascribed to caloric, and they seemed to prove that caloric exists in the electric fluid. This much however is evident, that the electric fluid is not caloric itself, otherwise it must heat those bodies through which it passes; but this is not the case: it appears also that in the electric sparks it is combined with some other substance which prevents it from communicating heat to bodies; and perhaps it is in a condition to do so only when it is disengaged, and becomes free by the electric matter being decomposed.

Whether this other substance may not be the matter of light, and whether during these experiments it passed through the sides of the glass, as no signs of any other matter on the glass could be observed, he could not decide. All the caloric disengaged was not, however, employed in the formation of inflammable air: a part of it remained free, and heated the tubes, which within five minutes were at a temperature equal to 150 degrees of Fahrenheit.



3. Experiments made to determine whether it was possible by electric sparks to decompose certain substances, or to change them in a sensible manner.—As Dr. Van Marum, in the years 1785 and 1787, was able to decompose nitrous and alkaline air by electric sparks, he wished to try the same experiment with other substances. For this purpose he employed tubes of from 13 to 14 inches in length, and from 3 to 4 lines in diameter, into the ends of which he introduced platina wires, and fused the glass around them. When the bodies to be examined required mercury, he filled the tubes with that substance, and introduced the bodies in such a manner that they floated about an inch over it. Above the bodies an inch of air was left, that the sparks might be conveyed to them with greater force; for he had been taught, by experience, that the shocks are of the utmost importance in experiments of this kind. However, he could not venture here to employ any atmospheric air; as this air, when decomposed, gives nitrous acid, which would have mixed with the products obtained. The fittest for this purpose were vital air and azot. When he tried such substances as attack mercury, the whole tubes were filled with the acid, and a platina wire was immersed in it, so that its upper end was an inch deep below the surface of the acid. This wire served instead of the mercury as a conductor.

Having introduced concentrated sulphuric acid into this last apparatus, and conveyed to it, for a quarter of an hour, positive or negative sparks, no signs of any change were observed. The case was the same when it was strongly heated or rarefied.

Fuming nitrous acid gave, in the course of five minutes, a column of two inches of an aëriform fluid; but in a quarter of an hour very little of it remained. It appears that the caloric of the electric matter had given the acid a gaseous form, but in itself no change could be perceived.

Common nitrous acid gave a column of air of half an inch, which disappeared also when the electric matter ceased to act.

Common fuming muriatic acid exhibited the same phenomena as the former. The hyperoxygenated did not produce

duce the least gas; it appeared that the caloric had no great tendency to unite itself with the oxygen.

Carbonat of potash, or fused salt of tartar, treated for a quarter of an hour over mercury with sparks, experienced no change.

Concrete volatile alkali gave, between mercury and air, so much gas that the whole tube was filled with it. In this case the product was partly inflammable air and partly azot; and it appears from these experiments that the formation of gas from both these component parts takes place as well in air as in vacuo.

Tincture of lackmus did not become red, though exposed to sparks for half an hour.

Professor Volta had requested Dr. Van Marum to convey sparks to fused saltpetre, in order to try whether a decrepitation would take place. This, however, was not the case; and, after cooling, the saltpetre did not appear to be in the least alkalised.

As oxygen separates itself from horn-silver in the light of the sun, Priestley first proposed to Van Marum to expose it to electricity; but no air could be obtained from it, either between the mercury and water, or in the Torricellian vacuum.

Solutions of silver, copper, iron, lead, and quicksilver, in nitrous acid, and of gold and tin in aqua-regia, did not give the least precipitate in a tube furnished with a platina wire. With silver, lead, tin, and quicksilver, a little æriform matter was observed, which however did not amount to above one-fourth of an inch, and was again immediately absorbed after the experiment.

6. Experiments which show that charcoal contains hydrogen.

These experiments were made in consequence of a visit from Landriani, on the 10th of November 1788. Lavoisier's combustion of charcoal in oxygen gas had proved merely that carbonic acid is produced from charcoal and oxygen gas; but he as little proved as any of the antiphlogistians, by a direct experiment, that the charcoal or the carbonic acid, obtained by its combustion in vital air, carried with it no water.



water. The fixed air in this experiment was obtained by heat from a mixture of charcoal-dust well dried and freed from air, and red precipitate exposed to heat. In order that the vessels might be freed from all moisture they were exposed to a strong heat, and the quicksilver employed in the apparatus was boiled. In order to try whether the carbonic acid obtained contained any water, strong sparks were made to pass through it; and the operator watched with great care to observe whether water would be produced, and whether a long spiral iron wire, employed in the apparatus, would be oxydated. The fixed air itself occupied in the tube the length of nearly 4 inches  $6\frac{1}{2}$  lines before being subjected to electricity, and the diameter of the tube was 7 lines. As soon as the sparks entered it, the operators saw with astonishment that the column of air gradually increased, and, after being electrified 16 minutes, the air in the tube occupied a space of 5 inches 1 line, which gave an increase of almost  $\frac{1}{10}$  of the whole. They then washed the fixed air in caustic alkali till its volume ceased to be lessened, and the residuum in the tube amounted to two inches. The flame of a taper being then applied to the aperture, this electrified residuum inflamed, and therefore showed that it consisted entirely of unmixed inflammable air. As this result did not coincide with what we are taught by theory, it was resolved to repeat the experiments, and to employ double care in order to drive off the moisture. But as greater attention was now paid to what took place during the revival of the mercury, it was observed that some vapour was deposited in the upper part of the flask employed for the experiment, as well as in the tube through which the produced air was made to pass. It was supposed on the first view that this vapour was sublimated mercury; but it soon formed itself into small drops of water, which increased in size, so that no doubt now remained respecting the production of water. The reduction was then suspended, and the whole apparatus was heated and dried as well as possible; but, on continuing the experiment, the drops again appeared. Now, as it was impossible that this water could arise from the moisture of the vessels, it seems proved to Van Marum that carbon not only

contains the base of carbonic acid, but that also of hydrogen gas. But even if this experiment had proved the existence of hydrogen in charcoal, it would not follow that the inflammable air thence resulting effects the reduction of metallic oxyds in the way understood by the Stahlans; it would show nothing more than that charcoal is not a simple substance\*; for, if it were so, it would not produce water, but would simply join the the oxyd and reduce it.

[To be continued.]

II. *Of Chemical and Mineralogical Nomenclature.* By  
RICHARD KIRWAN, LL.D. F.R.S. and P.R.I.A.

[Concluded from Page 179.]

ANOTHER inconvenience arising from denominations aiming to express the composition of the objects on which they are bestowed, is, that they often cannot express the proportion of the compounding ingredients without considerable embarrassment; this proportion nevertheless is a circumstance often of great importance, as it induces an important difference in their properties, and the embarrassment is greater when the compounding ingredients are numerous. To obviate the first, in one particular instance, the French school have very properly assigned different names to compounds holding different proportions of oxygen and possessing different properties in virtue of that difference of proportion, as *acids* and *oxides*; but several of the vegetable acids differ from each other only in the proportion of ingredients, which cannot, even if fully known, be expressed in detail on every occasion. So, to obtain a distinct knowledge of the various combinations of sulphur, or sulphur and hydrogen, with different bases, is of the utmost consequence to any one that wishes to obtain any insight into the phenomena presented by mineral waters during their analysis, or of the nature of other sulphurated compounds: on this obscure and intricate investigation most

\* It is well known that charcoal retains water with great obstinacy and it is extremely probable that, in spite of the care used in this experiment, some of that liquid had been left in the charcoal employed.



certainly the eminent abilities of Fourcroy, and the consummate skill and sagacity of Berthollet and his associate Welter, have thrown the clearest light; yet I must own that the terms employed by the two last to denote the different compounds appear to me very perplexing, though the best that could be chosen on the principles of the new nomenclature\*. I flatter myself, therefore, that a short explanation of them, and of the names I would wish to substitute in their room, will not be unacceptable nor out of place on this occasion.

*Sulphure, Sulphuret of the English.*

By this the French understand the compound of *sulphur singly* with any bases except hydrogen: these compounds I call *hepars* if unmetallic, but if metallic I apply their antient names.

*Hydrogene Sulphuré, Sulphurated Hydrogen.*

This complex denomination expresses the union of sulphur with hydrogen. I call it by its antient name, *hepatic air*.

*Souffre Hydrogéné.*

This term expresses the union of sulphur with hepatic air. The sulphur abounds, and it does not form a permanent air, but assumes an oily form. English neologists would, I presume, have called it *hydrogenated sulphur*: it is plain that this name does not express all the ingredients, for it is to hepatic air, and not merely to hydrogen, that the sulphur is united; I call it therefore *hepaticated sulphur*.

*Hydro Sulphure.*

Hepatic air is capable of uniting to various bases, and even precipitates sulphur from them if previously united to them; on these compounds Berthollet bestows the above denomination. The English, I suppose, would call them *hydrosulphurets*; I call them *hepatules*.

*Sulphure Hydrogéné.*

This denotes the union of hepaticated sulphur (*souffre hydrogéné*) with any basis: the English, I suppose, would call

\* See Ann. Chem. XXV. p. 230. and New Rox. Jour. III. p. 436.

these compounds *hydrogenated sulphurets*: this appellation does not express the composition. I employ the term *hepaticated* to express this composition. Berthollet has discovered that fixed alkaline hepars, when dissolved, or even moist, are always in this state, and consequently, that an alkaline hepar cannot exist but in a dry state.

In the case before us, we see the confusion and perplexity occasioned by a strict adherence to the rule recommended by Lavoisier \*, that a conformity or connection should be maintained between the names of a basis and its compounds: in some cases it is highly proper, and hence the different degrees of oxygenation of certain acids are very properly indicated by a slight alteration of the termination of the name of each, as *sulphureous* and *sulphuric*; but these names indicate only *extreme states*, the first only the smallest degree of oxygenation constituting an acid, and the last the state of *perfect saturation*: now, this same acid commonly occurs in neither of these states, but in a state participating more or less of both; these names, therefore, applied to it when thus circumstanced are false; and as in this intermediate state it has always been known by the name of *vitriolic acid*, I think this name should still be retained. Nay, the sulphureous acid is itself capable of two very different states, as may be seen in the 6th volume of De Machy's edition of Junker, p. 143. So the term *nitric* is very proper to denote the full saturation of the nitrous basis with oxygen. But the term *nitrous*, employed to denote the smallest degree of oxygenation necessary to convert this basis into an acid, is improper, as it has evermore conveyed a different idea. Hence I express this lowest extreme of oxygenation by the term *mephito nitrous*, as the radical primary basis may be called *mephite*, or mephitic air, instead of the new coined name *azot*. And the term *nitrous acid* may still denote, as it has evermore done, the mean state of oxygenation; that indeed in which it is usually found, and for which the French school have no name but that of one or other of the extreme states, which must therefore be falsely applied. The term *epinitrous air*, introduced by the learned and ingenious Dickson, may be used to denote what Doctor

\* Lavoisier, p. 72 and 73.



Priestley called *dephlogisticated nitrous air*, at least until its nature is better developed.

It is in vain that the authority of Bergman is invoked to countenance the suppression of the antient names of Glauber, Epsom, &c. rejecting only the ostentatious additions made to some of them, as *sal admirabile Glauberi*, &c. 4 Bergm. p. 257. It is true he affirms that *the best names are such as indicate the composition, or some essential property*; but of this sort he gives no example, nor rejects any old names merely on this account: on the contrary, where *expressive names* cannot easily be had, he tells us it is better to apply some that convey no determinate expression, p. 259; and this is often the case where different proportions or numerous ingredients are to be denoted.

Hence I am far from rejecting, but on the contrary applaud the ingenuity of the inventors of the terminations of *at* and *ite* to denote the different proportions of oxygen in the acids contained in different compounds, as *sulphats*, *sulphites*, *nitrats* and *nitrites*, &c. as such general names were undoubtedly wanting, and the old school afforded none. But the welcome admission of these does not require nor imply the dismissal of such of the old as were faultless, and enfranchised by prescription.

A highly valued friend suggested to me, that the use of the old names was a departure from the system on which the new denominations were founded. I replied, that systems were the creatures of convenience, and should be adhered to only as far as they promoted it; nitre, epsom, borax, &c. are much shorter (and equally well known) than nitrated potash or sulphat of magnesia; and Lavoisier himself preserves the name borax; he might as well have preserved that of epsom.

The fifth rule (very properly) laid down by Morveau with respect to names is, that they should be adapted to the genius of the language; consequently, if old names be retained, they should be employed in the true usual sense of their own signification in that language, and neither extended beyond it nor restricted within narrower bounds: on this ground I reject the term *potash*, employed to denote the vegetable fixed alkali

alkali in its purest state; for that name, both in English and French, has always denoted an impure alkali; but the purest alkali of this sort, having been formerly denoted by the name of *salt of tartar*, a name certainly improper, I substitute in its room the unexceptionable name *tartarin*, which, by its affinity to the former, easily suggests its signification, and is moreover attended with a smooth flowing adjective, *tartarinated*, which is often wanting. For the same reason I reject the name *ammonia* to express the *volatile alkali*, as the name *ammoniac* has always been employed to express the combination of a volatile alkali with an acid, and, if no particular acid was expressed, the muriatic was understood: instead of volatile alkali, which is a compound denomination, I substitute *volalkali*, whose signification cannot be mistaken. Its adjective is not, indeed, quite so happy: instead, then, of *volalkalised*, I use the word *fuliginated*, which easily indicates the same idea. These are the only new chemical names I employ.

The term *oxide* is also unsuited to our language, in which it naturally expresses the *bide of an ox*. In pronunciation they cannot be distinguished; in its stead I would use *oxat* or *oxidat*, and instead of *oxidized* I would substitute *oxidated*. The application of either of these terms to metallic substances in an oxidated state is generally superfluous, as such substances are already denoted, and known under the name of *metallic calces*. Guyton\* has lately proved that diamonds are the purest carbon; yet surely even the French school will not attempt to suppress that well-known name, and exchange it for carbon. Neither, I suppose, will they call charcoal an oxide of carbon, though proved to contain some portion of oxygen; and for the same reason I shall not exchange the well-known term *plumbago* for that of *carburet of iron*, though with respect to similar compounds of other metals the term *carburet* should be employed.

This system of conciliation the French school rejects with

\* It is with much regret and reluctance I mention this gentleman under this new name, as he was generally known, and gained immortal fame, under that of Morveau; hence I shall still use this in quoting his former works.



disdain. Guyton tells us, "it is so much more difficult to conceive, as it is an evident sacrifice of principles to habits \*;" as if the ground of their system of nomenclature were universally allowed, and afforded rules so strict and general as to enjoy the singular privilege of admitting no exception! as if there was no such thing as *principles of convenience*, or, if there were such principles, that they were to be sacrificed to mere speculative truths (if truths) of much less importance: a truly harsh, intolerant, and despotic maxim! as ill calculated to point out the most advantageous road to science, as the maxim that a straight line should always be followed, would be to insure us the best road to the summit of a mountain, though presenting in that direction a series of scabrous and abrupt precipices; and hence departed from by Lavoisier himself, as we have seen in the instances of *saltpetre* and *borax*, and indeed by the whole French school in the instances of *water* and *diamond*, as already mentioned: for common sense, in some instance or other, seldom fails of asserting its rights: yet Lavoisier tells us he was censured (by some chemical bigots) for this condescension. The principles of religion and justice are the only that can in no possible case yield to expediency.

Among many just reflections that occur in the preface to Lavoisier's celebrated elementary treatise of chemistry, there are some connected with this subject that appear to me not quite correct. Thus, p. x. (of the original) he tells us, "that the only way of avoiding these errors (unfounded hypotheses) is to suppress reasoning, or at least to simplify it as much as possible, as it proceeds from us, and can alone lead us astray; to try it always by the test of experiment, to preserve only the facts, which are the *data* given by nature, and which cannot deceive us; to seek for truth only in the natural concatenation of experiments and observations, as mathematicians arrive at the solution of a problem by the simple arrangement of the *data*, &c." From this paragraph we might be led to conclude that all reasoning should be banished from chemical investigation, or, at least, that only the simplest should be admitted; yet it may easily be shown that

\* Ann. Chem. XXV. p. 207.

the most signal instances of successful chemical investigation in the obscurest subjects, and the happiest display of chemical sagacity, are the results of very complicated reasoning. Such is Berthollet's theory of aqua regia, Berthollet and Welter's observations on hepatic air, Fourcroy's on hepatic waters, Vauquelin's theory of the mutual decomposition of nitrous air and the solution of vitriol of iron \*, most of Scheele's and many of Klaproth's analyses, and a few others.

The just arrangement to which mathematicians owe the easy solution of their problems, is itself the result of profound reasoning, as is evident in the formation of equations. But the modes of reasoning employed in the solutions of mathematical and chemical problems cannot properly be compared, the former being founded on the relation of identity or equality, and the latter on that of cause and effect.

Page vi. he tells us, "it is the series of facts that constitutes science." I should rather say, it was a knowledge of the relation that subsists between the facts that occur; but neither the facts themselves can often be discovered without much subtle reasoning, nor can they be marshalled in a luminous series without discovering the reciprocal relations of the component ingredients of compound substances to each other; a discovery which often requires an elaborate train of reasoning.

Lastly, both he and Morveau tell us that the memory of learners is singularly relieved by compound denominations expressing the component ingredients of each compound. In reply to which, I say, that the science is not to be charged with a cumbersome train of words merely to gratify the indolence of beginners. Are we then, on every occasion, to substitute the definition of words for the words themselves? Are we, in imitation of the Germans, to say a *bandshoe* instead of a *glove*? Helvetius has long since remarked, that every man of common understanding possesses sufficient power of memory

\* All foreign chemists are infinitely obliged to him for giving the old denominations of weights and measures instead of centimetres, &c. Chemistry aims at enlightening the world, and not Frenchmen alone; it should therefore speak a language universally understood, and shake off the yoke of national pedantry.



to retain the signification of most words in his own, and often of those of several other languages: chemistry and mineralogy together scarcely present two hundred appertaining to them alone; neither the sciences of astronomy, law, or medicine, afford fewer.

The fashionable rage of coining new words from the Greek, *without any necessity*, has been particularly baneful to mineralogy, inasmuch that foreigners, though well acquainted with the received terminology, cannot, without being versed in that language, understand the meaning of the new-fangled terms lately introduced. It is well known that the mineralogical knowledge of all Europe is chiefly derived from the Germans and Swedes, whose nomenclature is in most instances the same, and where any ambiguity has arisen it has been removed by the exertions of Werner. His nomenclature, where not too discordant with the language, or at open variance with the received technical names of other countries, should therefore, for the sake of precision and uniformity, be universally preserved.

Abbé Haüy, who is now preparing a treatise of mineralogy, of which the highest expectations are justly entertained, since, in addition to his own superior intelligence and profound physical knowledge, he is assisted with the chemical abilities of Guyton, Vauquelin, Descotilles, and many others, and the extensive researches of Dolomieu, seems convinced of the propriety of retaining the received terminology, at least for the present, with the limitations and restrictions above mentioned; for, in his prefatory discourse, *Journal des Mines*, XXVII. p. 224, he tells us, “that with respect to minerals of the first class (that is, consisting of mere earths), he left them those names which they had hitherto borne, and did not take the liberty of imposing new ones, except in cases of necessity, as where a new species occurred, either formerly unknown, or confounded with one of a different nature. We have altered such names only as were intolerably ambiguous.”

Yet after this declaration we find the following new-coined names:

*Telefia*, for oriental rubies, sapphires, and topazes. Thus sapphire is called *blue telefia*, &c.—*Cymophane*, for the chry-

soberil of Werner.—*Amphibole* for crystallised hornblende.—*Pyroxene*, also crystallised hornblende; at least I have reason to think so, for of the eleven external characters of both, given by La Metherie, there is only one in which they absolutely differ, that is, the electrical.—*Staurotide* for stauro-lite, the name very properly imposed by La Metherie instead of the compound name it bore before.—*Axinite* I believe to be only a variety of thumerstein.—*Actinote* for actinolite, the termination *lite* most properly denoting a stone.—*Thallite* for delphinite, the name already given it by Saussure.—*Idocrase* seems, as I conjecture (seeing no exact account of it), a variety of olivin.

I pass over several new names, as *euclase*, *dioprase*, *chabasie*, &c. as they may possibly denote new species, which I wish were settled in conjunction with Berthout or Van Buch, or some of the Wernerian school. Yet even these denominations, we are told, are only *provisional*, being hereafter to be altered as analyses may require.

I must here add, by way of note, that C. Guyton, in reviewing the first volume of my Mineralogy, Ann. Chem. p. 105 and 106, has fallen into two mistakes, which I am persuaded his candour will prompt him to acknowledge. The first is, in stating that my experiments on the fusibility of different stones and earths were made in limestone vessels, whereas they were in Hessian crucibles. The second is, in stating the alleged infusibility of barytic earth and lime as the result of my experiments, whereas I expressly quoted Lavoisier, having myself made no experiments on such mixtures. (See Mem. Par. 1783, p. 599 and 600.) Other writers have frequently imputed to me mistakes of Bergman, though I expressly quoted him.



III. *A brief Examination of the received Doctrines respecting Heat or Caloric.* By ALEXANDER TILLOCH. Read before the Askesian Society, December 1799.

[Concluded from Page 126.]

I SHALL now endeavour to prove that the position which maintains that “heat, when in chemical union in bodies, cannot be made to manifest itself but by the action of some new chemical affinity, does not hold universally, and therefore may fairly be questioned.

Gases, as every one knows, are formed by the solution of certain bases in caloric. (Whether light be also an ingredient, or whether light be a modification of heat, or *vice versa*, affects not the present argument.) The base or bases, and the caloric, are chemically united, and therefore the caloric, according to the received doctrine, is *latent* in the gas; *it is not cognisable by any external sign or organ of sense.* I shall not stop to insist here upon the large increase of volume acquired by the base so dissolved—an increase cognisable by our organs of vision, and also tangible if the gas be received in a bladder, having already used a similar argument when speaking of the conversion of water into steam—but proceed to observe that this *chemically combined caloric* may be, partially at least, separated from its base *by means merely mechanical.*

To suppose that any body can by mechanical force be reduced into a smaller compass without any portion of its original ingredients being thereby driven out of the mass, would be an absurdity. When we attempt to compress in a metallic vessel, of a known capacity, a quantity of any gas equal in volume to twice that capacity, we, by the mechanical force employed with that view, absolutely cause a portion of one of the ingredients in the composition of the gas to percolate through the materials of which the vessel is made (every substance in nature being pervious to heat). That ingredient, that substance, is heat. The vessel in this case performs the office of a filter, suffering the heat to pass, but keeping back the other ingredients; and the quantity of heat passed off in

this case will be, if denominated by its volume, *exactly of the bulk of the capacity of the vessel*. If the vessel has been charged with a quantity of gas equal in bulk to *three times* its own capacity, the heat forced out will be *equal in bulk to twice that capacity*; and so of any other proportion of charge.

Here, then, the same effect takes place as when we confine steam; the means are mechanical in both; the heat is squeezed out through the sides of the vessel by the force employed, as water is squeezed from a sponge by pressure applied to it. Was the water in the sponge of a different nature before and after being expressed, first latent and then sensible? If it would be absurd to say so, why should we suppose heat, when dislodged by similar means, to have undergone any change as to its nature, and to have been *latent* before but *sensible* after? meaning thereby that its characteristic properties were different before and after the process.

But have we any experiments that prove heat to be actually separated by mechanical force from substances in which it is held to be *latent* or *in chemical union*? When the condenser of an air-gun, or any other proper vessel, is charged with more than one atmosphere, the materials, as the process goes on, become of a higher temperature than the surrounding bodies\*, and heat is given off to them. This shows that heat is expressed from the contained air; and that, not being able to pass freely, but forced to pass more or less slowly, according to the conducting power of the materials of the vessel, it is partially accumulated in the materials, and then from them passed off to surrounding bodies to restore equilibrium according to the general law.

If the wall of separation, which forces the confined gas to remain charged with a smaller quantity of heat than its constitution would permit, but for the violence employed, be removed, what takes place? The gas instantly seizes upon a portion of the caloric employed in maintaining the common temperature of the surrounding bodies, fitted to its own capacity in its unrestrained state, and in consequence resumes its original volume. This plainly appears to be the case,

\* See Dr. Darwin's paper on this subject, Philosophical Transactions for 1788.



for any substance placed before the cock when it is opened is reduced in temperature. Water so placed will even be converted into ice, in consequence of a portion of its heat being violently seized upon by the gas, and more quickly taken into union with it than the privation experienced by the water is compensated for by surrounding bodies in the passage of caloric from them to restore the equilibrium.

I am aware that it may be objected against this experiment, indeed I have heard the objection offered, that the friction of the forcing syringe must excite or accumulate a considerable portion of heat in the materials of which it is made; that the air, passing instantly from it into the condensing ball, must necessarily carry a portion of the heat along with it, having no intermediate bodies to come in contact with; and that therefore we have no evidence of heat being in this case expressed from the compressed air.

Some force might be allowed to this objection, if the air, when liberated, had no effect in reducing the temperature of the bodies exposed to it; but, as it has the power of taking from them a portion of their heat, and of reducing them below the common temperature, the objection falls to the ground: for, if the heat found to be accumulated in the materials of the condensing ball was only a consequence of its containing air, *made hot by passing from a cylinder heated by friction*, the air when liberated should *blow hot*, and *raise* the temperature of the bodies exposed to the stream instead of *lowering* it, as we find it does. The reasoning here is conclusive, but any person may also have the evidence of a direct experiment. Expose the ball of an air-gun, charged only with one atmosphere, to the heat of a common fire till the temperature of the ball be raised a few degrees: the contained air will now be in a situation to expand itself as soon as the valve is opened; for we know that the air has been heated by the ball. Expose a thermometer to the aperture as the air escapes, and the thermometer will indicate an increased temperature.

I have heard some object to the inference from a condensing ball being heated when charged, by questioning the fact, for “they have often applied their hand to it without  
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perceiving

perceiving any such effect." It may be true that a *thick* ball of small capacity will not give any sensible indication of heat to a *hand* applied to it, or even to a sluggish thermometer, which can only touch it in one point; for, where a *large mass* of materials is ready to receive the heat as it is expressed from the air, the effect may not be immediately obvious: it would be a long time before a large piece of ordnance could be made sensibly hotter by applying the flame of a taper to it, but who would therefore deny that heat has passed into the metal? Instead of applying their hands to the condensing ball, let them, as before stated, place a thermometer in the stream of the air when liberated, and they may receive complete evidence that the air demands a portion of caloric to bring it into equilibrium with the surrounding bodies, which could not possibly be the case if it had not previously suffered some privation.

I shall here briefly state the results of an experiment undertaken for the express purpose of ascertaining whether there had been any mistake in the facts reported by others respecting the phenomena which accompany the compression of air\*.

By means of a powerful syringe A, Plate VI. fig. 1. (23 inches long and 2 inside diameter) made fast to one end of a table and to the floor of the room, the vessel B, constructed of tinned copper, and made fast to the other end of the table, was charged with as much air as could be forced into it by the strength of one man. The temperature of the room was 59°. By the condensation of the air, the thermometer C, introduced into a tube soldered into the vessel B (and open at its exterior extremity), was raised seven degrees. On exposing the bulb of another thermometer to the orifice of the cock D, which was then opened to dis-

\* For the execution of this experiment I am indebted to the London Philosophical Society. Having stated to that Society the fact, which I wished to see ascertained, the subject was taken up with that alacrity and zeal which characterises all its proceedings. The apparatus was prepared by Mr. Farley, experimenter to that institution, and the experiment was made in the presence of the Society on the 18th of November 1799.



charge the air, that thermometer sunk to nine degrees below the temperature of the room, or sixteen degrees below the point to which the other had been raised by the expressed heat.

It cannot be urged that in this experiment the air passed immediately from the syringe into the air-vessel, for it had to traverse a tube interposed between them of about four feet in length.

Now, when it is considered that the thermometer was in contact with the vessel B in only one minute point (for the tube in which it was inserted, and against the side of which the bulb rested, was more than three times the diameter of the bulb), and that the whole surface of B was above 50 square inches; and when it is also recollected that only a small portion of the effect which the air issuing from D was capable of producing could manifest itself by means of the thermometer exposed to its action (as the thermometer was obliged to be kept at some distance to prevent its being broken by the discharge of the air); some idea, though not an adequate one, may be formed of the mass of heat that must have been thrown off from the whole surface of B in the process of charging that vessel \*.

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\* Subsequent to the delivery of the present paper to the Society, I received Mr. Mushet's interesting communication, which was afterwards inserted in the *Philosophical Magazine* for February 1800. It contains some curious facts which confirm and illustrate the results obtained from this experiment. Speaking of the effects produced in the blast-furnace by the nature, compression, and velocity of the air used, he observes that the compression always occasions an increase of temperature. On entering a blowing cylinder immediately after stopping the engine, he finds the thermometer rise from  $15^{\circ}$  to  $17\frac{1}{2}^{\circ}$  higher than the surrounding atmosphere. A thermometer held in the middle of the current of blast was found to be reduced below the common temperature as much as the cylinder was raised above it. In some cases, when the common temperature is about  $54^{\circ}$ , the blast, as it issues, will sink the thermometer  $2^{\circ}$  or  $3^{\circ}$  below the freezing point.

He states another fact (see also Mr. Roebuck's paper, *Philosophical Magazine*, Vol. VI. p. 324.), which completely does away every idea of any part of the effect being derived from friction. At some iron works air-vaults of from 60,000 to 70,000 cubical feet in content are employed for  
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The results obtained by the preceding experiment, though only a small part of the effect could be appreciated, are sufficient to ascertain the fact it was meant to establish—that the doctrine which asserts heat to exist in a latent state in the atmosphere, and consequently to be not separable from it without a chemical decomposition of the air, cannot be true, otherwise mechanical pressure and chemical decomposition mean the same thing, which the advocates for the existence of heat in two distinct states will hardly maintain.

But the effect would be much more striking, and the quantity of heat could even be comparatively estimated by employing in the experiment such an apparatus as would enable us, in some measure, to intercept the heat as it passes off from the whole surface of the air-holder, and accumulate it in some liquid.

After thinking on various contrivances, with this view, a method of constructing such an apparatus occurred to me, a description of which, though I have not yet had time to carry it into execution, I beg leave to lay before the Society.

#### *Description of a Gaso-Calorimeter.*

The gas-holder A, fig. 2. of any capacity, say a quart, constructed of tinned copper or any other metal, well soldered and riveted at the joinings, should be made double, or, in other words, the vessel, properly called the gas-holder, should be covered with another of the same shape made of thin metal, (as tinned iron) in such a manner that the two may not touch each other except at the lower part, where they are fitted on, and soldered fast to one common collar. By this means a space is left between the exterior surface of the gas-holder and the interior of the cover, to be filled with a fluid for the purpose which shall immediately be stated. The interior vessel, which is to receive the gas, is open at the collar, and into this collar is fitted the cock B, made of glass, because of the mercury to be employed in the experiments, and the purpose of equalising the blast. In these the increase of temperature is more sensible than in the blowing cylinder. It takes place at a considerable distance from any mechanical friction, and is therefore evidently produced by heat extricated from the air.

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for the convenience of seeing. The exterior vessel has an opening at the top C, into which is fitted a glass tube CD. Through this tube (or before fixing the tube in its place) fill the space between the interior and exterior vessel with any coloured liquid.

If this double vessel be now filled with, and immersed in, warm water, it is plain that the heat thus communicated to it will expand the coloured liquid contained between the interior and exterior surfaces, and make it rise to such a height in the glass tube as shall correspond to the temperature of the water in which it is immersed. By employing water of different temperatures the different degrees of heat, as ascertained by an accurate thermometer, may be marked upon the tube, or on a scale attached to it; and thus the vessel itself will, in fact, become a large hollow thermometer.

To the lower end of the cock B attach another vessel E, made of cast iron, furnished with two other cocks, *a* and *b*, and connected with the vertical tube FG, made of iron (a series of gun barrels properly joined to each other) or of glass, secured by a case of wood, with cement interposed between it and the glass.

Provide also a vessel of the same capacity as the internal dimensions of the gas-holder A. This vessel may be called the measure.

From the above short description the construction of the gaso-calorimeter, its use, and the way of operating with it, may be easily conceived.

Open the cocks *a* and F, and pour mercury into the funnel at G. It will rise in E, driving the air before it, which will escape by the cock *a*. Having in this way filled the vessel E with mercury up to the cock B, shut the cock F, place the measure before mentioned below the cock *b*, and by that cock draw off one measure of the mercury. It is obvious that by so doing one measure of air will at the same time pass into E through the cock *a* \*. The cocks *a b* being now shut, pour mercury into the tube FG to any convenient height; open the cock B, and then the cock F. The mea-

\* Any other gas may be introduced by connecting this cock with a pneumatic apparatus.

sure of air contained in the vessel E, having now no way to escape, will, by the ascent of the mercury in E, be forced to pass through the cock B into the gas-holder A. As soon as this is perceived, by the mercury showing itself in the glass cock B, shut that cock.

The gas-holder will thus become charged with two atmospheres, and that in a way so expeditious as to allow but little time for the escape and loss of caloric, especially if the tube FG be of large diameter, and if the vessel E have been previously lined, and the vessel A covered over with some bad conductor of heat. The expressed heat must then pass through the sides of the gaso-calorimeter, and there, exercising its action on the interposed thermometric fluid, must make it ascend in the tube CD to a height proportioned to the heat thus communicated to the fluid \*.

Another atmosphere may be introduced by shutting the cock F, opening *a* and *b*, and withdrawing another measure of mercury as before, and then, by mercury poured into GF, making the air to ascend into A, in the manner already described †. Then note again the height to which the thermometric fluid rises in CD; and so of a fourth or a fifth atmosphere; observing that the height of the column of mercury in FG must always be proportioned to the number of atmospheres to be introduced.

The quantity of coloured fluid contained between the exterior and interior of the double vessel A, should be determined by weighing the quantity introduced before graduating the tube CD. By this means it will be possible to determine not merely how many degrees the heat expressed from the air raises the fluid in the tube CD, but to how many ounces of the fluid it communicates that degree of temperature. The weight of the materials of which the double vessel

\* As mercury is found to be a good conductor of heat, it would be advisable to have a little oil, or some other liquid, in the vessel E, before the first introduction of the mercury. The oil would rise on its surface, and the interposed stratum rising with the mercury prevent its contact with the air driven before it into the vessel A.

† Or two or more atmospheres may be forced up at once, if the vessel E be made of sufficient capacity.



is made, should be also determined, that it may be known how many ounces or pounds of metal have also been heated to the same degree.

The converse may be exhibited by the same apparatus by a different mode of operating. Detach the vessel A from the vessel E; invert it, and fill it with water, previously deprived of air by boiling or by the air-pump, and, having shut the cock B, again invert it and join it to the vessel E. Fill the latter with mercury in the manner that has been already described, and to the cock *b* adapt an iron tube, of 31 or 32 inches in length, to descend vertically into a basin of mercury. If the cocks B *b* be now opened, all the others being shut, mercury will descend from E, through the tube fitted to *b*, into the basin in which the lower extremity of the tube is, and the water will pass from A into E, leaving a vacuum in A. When the water has all passed out of A, shut the cock B.

If one atmosphere be now introduced into E, and, the cocks *a* and *b* being shut, be compressed into a half, a fourth, or a fifth of its original volume, by means of a proportionate column of mercury in the tube FG, heat will be expressed from the air, which must pass off through the sides of the vessel E. Allow the apparatus to stand for some hours, till every thing connected with it has come to the common temperature. Then open the cock B, and the compressed air, finding itself at liberty, will pass up into the vacuum in A, where it will find space to expand itself to its natural volume, the quantity employed being one measure, or, in other words, equal exactly to the capacity of A. The mercury should also be allowed, by opening the cock F, to ascend till it reach the cock B. But the air, in resuming its original volume, will demand heat from the materials of which A is constructed, and from the thermometric fluid of A, faster than they can receive it from the atmosphere and contiguous bodies, especially if defended by a covering made of a bad conducting substance; and, as a necessary consequence of this, the thermometric fluid will fall in the tube FG.

If heat be thus detected passing from or into air by mere

pressure, or the contrary, and that it will we have already sufficient evidence, of *what kind is the heat?* According to the received doctrine, it is neither *sensible* nor *latent* heat; for that heat which, united to oxygen and azot, forms atmospheric air, is *not sensible*, and that which raises the temperature of bodies is *not latent*: but this heat *constitutes* one-half, two-thirds, three-fourths, or four-fifths of *atmospheric air* (according to the number of atmospheres compressed into one), is a constituent *chemical* ingredient of the atmosphere, and therefore *latent heat*: and yet it raises the temperature of other bodies, without being separated from its union with the oxygen and azot *by the exercise of any chemical affinity*; therefore it is *sensible heat*.

The doctrine seems evidently to stand opposed to itself; or shall we, to avoid this conclusion, say that heat has a third mode of existence, in which it may be called SENSIBLE-LATENT CALORIC! Absurd as such a position might be, it would not be more so than the doctrine of latent heat, taking that term in its common acceptation.

I meant to have troubled the Society with a few further remarks on this interesting subject, but, having already encroached further upon its time than I at first intended, must defer them for the present.

To conclude: The doctrine of latent and sensible heat appears to me to have arisen from a want of due attention to the facts established by the veteran Black respecting the different capacities of bodies for heat. This eminent chemist was the first who proposed any thing rational on this subject, and, when the beautiful simplicity of the first principles which he established is duly attended to, it appears wonderful to me that the simple and permanent structure that might have been reared upon them should have escaped his sagacious observation. Having established this incontrovertible fact, that *different* substances have different capacities for heat, this truth of itself necessarily embraced another, which, though it could not possibly escape observation, has never been applied as it ought; namely, that in every chemical combination we effect we are altering the capacities of bodies for heat, and consequently deranging the equilibrium; for



the product *differs* in its capacity from the ingredients; and the same holds in decompositions. Had this been properly attended to, it would have been found perfectly sufficient, when taken along with that property by which heat tends to equilibrium, to explain the passage of heat from substances to other substances, without ever once supposing the heat changed in its properties. Heat seems to act uniformly, and its effects depend always on its quantity (not kind) compared with the capacity of the body into which it enters: but it is continually bandied about, as it were, by the constant changes that are passing upon bodies, by which their capacities for receiving or holding it are altered; so that it is in a constant state of influx and efflux in bodies, and there is going on a constant adjustment, as it were, of the differences existing among them, each requiring its own share of the common stock, and giving off, receiving, or merely transmitting heat, according to circumstances. Bodies are continually undergoing change by the action of heat. This is admitted on all hands. Is it necessary then to look for a change in the *agent* as well as the patient? In many respects its action, as we have before observed, may be illustrated by that of water. Different substances require different quantities of water to dissolve them; and different substances require different quantities of heat to dissolve them. The property which different substances have to take in different quantities of water, may be called their capacity for water: but who ever talks of a certain quantity of water, when diffused among any number of substances in proportion to their capacities, being *latent water*? or, when an interposed hygrometer is affected by its passage from one substance to another, of its being then *sensible* or *free*?

IV. *Reflections on the Theory of the Infinitesimal Calculus.* By C. CARNOT, *Ex-Director of the French Republic, Minister of War, and Member of the National Institute, Paris 1797.* Translated from the French, and illustrated with Notes, by WILLIAM DICKSON, LL. D.

*To the Editor of the Philosophical Magazine.*

DEAR SIR,

THE mathematical readers of your excellent publication need not be told of the importance of the infinitesimal calculus, or of the assistance it lends to every part of natural philosophy, which falls not within the province of chemistry. That calculus is practically the same with what we call the method of fluxions, except in name and notation. But the theories of the two methods are different; or, rather, those theories are only different ways of illustrating one and the same method under different names and symbols. The fluxionary theory, as delivered by its inventor, the incomparable Newton, is confessedly the most accurate; but the infinitesimal, otherwise called the differential theory, is generally thought, I know not for what reason, to be the most easily understood\*. As it is often advantageous to consider a subject in different points of view, it was perhaps fortunate that M. Leibnitz claimed this discovery, as he was, of course, obliged to give it a new explanation; and thus we have the ideas of two of the most acute men who ever existed, on one of the most extraordinary and sublime speculations which ever exercised the human intellect.

\* "We trust," said the excellent apostle, with infinitely more depth and propriety than appears at first sight; "we trust that we have a good conscience." I trust that I understand something of Newton's fluxionary theory; but I never could understand the differential, as delivered by Leibnitz, in the *Acta Eruditorum* of Leipzig, for October 1684, of which the learned and ingenious Mr. Raphson has inserted a translation in his *History of Fluxions*. I believe most impartial persons, who try to read that perplexed and perplexing piece, will be far from accusing the historian of severity, when he says, that it contains "far-fetched, symbolising, insignificant novelties," and that the notation is "less apt and more laborious" than that of the real inventor.

In



In the essay, of which the following is a translation, the two theories are blended, and so clearly explained, that those who think the *pure* fluxionary theory, even as facilitated by Mr. T. Simpson, in his excellent "Select Mathematical Exercises," still somewhat difficult, may derive instruction from the author's new and perspicuous way of treating the subject. This theory, like other mathematical theories, being of an abstract nature, may perhaps, after all, be more easily conceived than expressed. And, as happens in the practice of arithmetic, there may be persons who are expert in the application of fluxions, but who have never been able fully to satisfy themselves as to the demonstrable validity of the theory. It should however be remembered, that all rational practice entirely depends on sound theory; and, to use the words of the great Maclaurin \*, in proportion as the "general methods are valuable, it is important that they be established above all exception; and since they save us so much time and labour, we may allow the more for illustrating the methods themselves."

Those who, with a competent knowledge of geometry, algebra, and the conic sections, attentively peruse the following essay, will perhaps wonder why a subject capable of such clear illustration has been represented as so difficult. The performance, though obviously not without faults, is, upon the whole, well worthy of the attention of such mathematicians as I have alluded to. The translation is as literal as the idioms of the two languages would bear; and I have annexed notes where I thought they would be useful. Though I decidedly prefer the fluxionary notation, as well as theory, to the differential; yet in the *text* I have retained, among the other symbols of the original, all the *d*'s, and the *dd*'s, and the *ddd*'s and the *dddd*'s, to which the foreign mathematicians are so *partial*, as being most congenial with the ingenious author's manner of considering his subject.

I am,

Dear Sir,

Sincerely and respectfully yours,

W. D.

\* In the Introduction to his Treatise of Fluxions.

*Advertisement by the Author.*

Some years ago, the author of the following reflections reduced them to the form in which he now presents them to the public. He is at present charged with business so very important as to forbid his resuming his former studies; but, as every circumstance announces that the mathematical sciences are about to soar to a new elevation, it is believed that some advantage may result from the publication of a memoir in which the theory of the differential calculus is discussed at large, and with precision, and in which are united the different points of view in which that theory has been considered.

*Subject of the following Dissertation.*

I. No discovery ever produced so happy and so sudden a revolution in the mathematical sciences as the infinitesimal analysis; nor hath any improvement furnished us with such simple and efficacious methods of arriving at a knowledge of the laws of nature. By decomposing, so to speak, magnitudes into their constituent elements, that analysis seems, as it were, to have detected their internal structure and organisation. But as all extremes elude the cognisance of the senses and the powers of the imagination, we have been able to form but an imperfect idea of those elements, a singular kind of beings, which must sometimes be treated as real quantities, sometimes as absolute nullities, and which seem, by their equivocal properties, to hold a middle place between magnitude and zero, between existence and nothing\*.

Happily this difficulty hath not interrupted the progress of the discovery. There are certain primitive ideas which are always somewhat clouded in the imagination, but whose first

\* I here speak in conformity with the vague ideas which we commonly entertain of infinitesimal quantities, when we have not been at the pains to examine their nature. But in truth, nothing can be more simple than the notion of infinitesimals. Indeed, to say that a quantity is infinitely small, is precisely to affirm, that it is the difference of two magnitudes which have for their limit the same third magnitude, and nothing more is meant. The idea, then, of an infinitesimal quantity is not more difficult to conceive than that of a limit; but it has besides, as is universally allowed, the advantage of leading to a much more simple theory.

consequences,



consequences, when once fairly deduced, open a vast field, which may be easily traversed. Such appears to be the idea of infinity, of which many geometers, who *perhaps* were never able to fathom it, have made the most happy use \*. Philosophers, however, unable to content themselves with so vague an idea, have been wishful to ascend to principles; but in doing this, they soon found themselves divided in opinion, or rather in their manner of viewing the objects of their research. My design in this performance is to reconcile those different points of view, to show my readers their relations, and to propose new ones; and I shall think myself well rewarded for my trouble, if I should succeed in throwing some degree of light on a subject so interesting.

*The Origin which the Infinitesimal Calculus might have had.*

2. The difficulty frequently experienced in accurately expressing by equations the different conditions of a problem, and in resolving those equations, *might* have given birth to the first ideas of the infinitesimal calculus. When, indeed, it is too difficult to give an exact solution of a question, it is natural for the analyst to seek the means of approximating to it as nearly as possible, by neglecting the quantities which embarrass the combinations, if it be foreseen that such quantities, on account of their small value, may be neglected, without producing any sensible error in the result of the calculation. Thus, for example, as the discovery of the properties of curve lines is very difficult, they have been considered as polygons

\* Certainly no geometer, or metaphysician either, was ever able to fathom quantities infinitely great or infinitely small; I had almost said, even in the restricted sense of the mathematicians, who define them to be quantities greater (or less) than any assignable quantities. Yet the metaphysicians can prove that such quantities must necessarily be received among the other *entia rationis*; and the mathematicians demonstrate many of their relative powers and properties; just as the chemists, *mutatis mutandis*, exhibit to the senses many of the properties of natural bodies, though they do not pretend to have any idea whatever of their intimate essences. On the difficulty, or rather impossibility, of conceiving, and the necessity of admitting, Eternity and Infinity, see Dr. Samuel Clarke's excellent Demonstration of the Being and Attributes of God, page 3, *et seqq.* 10th edition. W. D.

of a very great number of sides. If, indeed, we conceive a regular polygon to be inscribed in a circle, it is evident that those figures, though they be always different, and can never become identical, yet resemble each other more and more as the sides of the polygon increase in number. The circumferences of those figures, their surfaces, the solids formed by their revolutions about a given axis, the analogous lines drawn without and within them, the angles formed by those lines, &c. are, if not respectively equal, at least the nearer to equality the more the number of sides augments. Hence, by supposing the number of sides to be very great, we may, without any sensible error, attribute to the circumscribing circle, the properties which we have discovered to belong to the inscribed polygon.

Farther, the sides of this polygon evidently diminish in magnitude in proportion as their number is augmented; and consequently, if we suppose the polygon to be really composed of a very great number of sides, we may also affirm that each of those sides is really very small.

This being understood, if, in the course of calculation, a case happen to occur in which many operations would be much simplified by neglecting, for example, one of those sides, which is small in comparison with a given line; that is, by employing in the calculation that given line instead of a quantity equal to the sum of that given line and the little side in question, it is clear that this may be done without inconvenience; for the resulting error must be extremely small, and it will not be worth while to inquire into its value.

3. For example, if it were proposed to draw a tangent to a given point  $M$ , of the circumference of the circle  $MBD$  (fig. 1).

Let  $C$  be the centre of the circle, and  $DCB$  the axis. Suppose the absciss  $DP = x$ , the corresponding ordinate  $MP = y$ , and let  $TP$  represent the subtangent required.

In order to find that subtangent, let us consider the circle as a polygon of a very great number of sides. Let  $MN$  be one of those sides, and be produced till it meet the axis. It will then evidently be the tangent in question, because it falls not within the polygon. Moreover, let fall the perpendicular



pendicular  $MO$  upon  $NQ$ , which is parallel to  $MP$ , and put  $a$  for the radius of the circle. We shall then evidently have

$$MO : NO :: TP : MP; \text{ that is, } \frac{MO}{NO} = \frac{TP}{y}.$$

On the other hand, the equation of the curve for the point  $M$  being  $yy = 2ax - xx^*$ , the equation for the point  $N$  will be

$$(y + NO)^2 = 2a(x + MO) - (x + MO)^2;$$

and subtracting the former equation from the latter, and reducing, we have

$$\frac{MO}{NO} = \frac{2y + NO}{2a - 2x - MO}; \text{ but we before found}$$

$$\frac{MO}{NO} = \frac{TP}{y}; \text{ therefore } \frac{TP}{y} = \frac{2y + NO}{2a - 2x - MO},$$

and multiplying by  $y$ , we have  $TP = \frac{y(2y + NO)}{2a - 2x - MO}.$

If, then,  $MO$  and  $NO$  were known, we should have the value of  $TP$ , the subtangent required. Now the quantities  $MO$  and  $NO$  are very small, being each of them less than the side  $MN$ , which, by the supposition, is itself very small. We may therefore, without any sensible error, reject those quantities, in comparison with the quantities  $2y$  and  $2x - 2a$ , wherewith they are connected. The equation will then be reduced to  $TP = \frac{2y^2}{2a - 2x} = \frac{y^2}{a - x}$ , the value of the subtangent sought.

*The Infinitesimal Calculus may, in this View, be considered as a simple Method of Approximation.*

4. If this result be not absolutely exact, it is at least evident that, in practice, it may pass for such, because the quantities  $MO$  and  $NO$  are extremely small. But a person who has no idea of the doctrine of infinites, will perhaps be surprised to be told that the equation  $TP = \frac{y^2}{a - x}$ , does not barely make a near approach to the truth, but that it is really and perfectly accurate. Of this, however, he may easily satisfy himself by

\* For, in the circle,  $2a - x : y :: y : x$ .

investigating the value of  $TP$ , on the principle of the tangent being perpendicular to the extremity of the radius; for it is evident that the similar triangles,

$$CPM, MPT, \text{ give } CP : MP :: MP : TP;$$

and therefore  $TP = \frac{MP^2}{CP} = \frac{y^2}{a-x}$ , as before.

5. As a second example, suppose we are to find the surface of a given circle. Let us still consider that curve as a regular polygon of a great number of sides. The area of any regular polygon is equal to the product of its circumference into half the perpendicular let fall from the centre on one of its sides. The circle, therefore, being considered as a regular polygon of a great number of sides, its surface ought to be equal to the product of its circumference (that is, the sum of very numerous sides) into half the radius; a proposition as exact as the other.

6. However vague and indeterminate the expressions *very great* and *very small*, or others of the same nature, may appear, we see, by the two preceding examples, that they are not unusefully employed in mathematical combinations, and that the quantities signified by these expressions may afford much help in facilitating the solution of divers questions which may be proposed. For, the notion of such quantities being once rightly understood and admitted, all other curves, as well as the circle, may be considered as polygons of a great number of sides; all surfaces may be divided into a great number of fillets or zones; all bodies may be resolved into corpuscles; and, in a word, all quantities may be decomposed into particles of the same species with themselves. Hence arise new relations and combinations; and we may easily judge, from the examples already adduced, of the resources in calculation, which the introduction of such elementary quantities must afford.

7. But the advantages of admitting these quantities is still more considerable than could, at first sight, have been expected. For we have seen, in the examples proposed, that what was considered, at the outset, as a simple method of approximation, conducts us, at least in certain cases, to results perfectly accurate. It becomes interesting, therefore,



to be able to distinguish the cases in which this accuracy takes place, and thus to convert this method of approximation into a calculus perfectly exact and rigorous; and such is the object of the infinitesimal analysis.

8. Let us first see, then, how it came to pass that, in the equation  $TP = \frac{y(2y + NO)}{2a - 2x - MO}$ , found in article 3d, though we neglected  $MO$  and  $NO$ , the justness of the result was not affected; or rather, how the result became exact from the suppression of those quantities, and why it was not exact before that suppression.

Now, the reason of what happened in the solution of the above problem will easily appear, on remarking that the hypothesis on which that solution is grounded, is false; it being absolutely impossible that a circle can be a *true* polygon, whatever be the number of its sides; so that there must result from that hypothesis an error of some kind in the equation  $TP = \frac{y(2y + NO)}{2a - 2x - MO}$ . But the result  $TP = \frac{y^2}{a - x}$  being nevertheless exact, as was proved by the comparison of the two triangles  $CPM$ ,  $MPT$ , we found that  $MO$  and  $NO$  might be neglected in the first equation. Indeed these quantities *ought* to be neglected, in order to correct the calculus, and to destroy the error which had arisen from the false hypothesis first assumed. The rejection of quantities of this nature is not barely allowable, but is absolutely necessary in such cases, as being the only manner of expressing accurately the conditions of the problem.

*The Accuracy of such Results is owing to a Compensation of Errors.*

9. The accurate result  $TP = \frac{y^2}{a - x}$  has been obtained then, only in consequence of a compensation of errors; and this compensation may be rendered still more evident, by treating the example above given in a manner somewhat different, namely, by considering the circle as (what it really is) a true curve, and not a polygon.

For this purpose, from the point  $R$  (fig. 1.) taken arbitrarily

rily at any distance from the point  $M$ , draw the line  $RS$  parallel to  $MP$ , and, through the points  $R$  and  $M$ , draw the secant  $RT'$ ; then we shall evidently have  $T'P : MP :: MZ : RZ$ , and therefore  $T'P$ , or  $TP + T'T = MP \frac{MZ}{RZ}$ . Now,

if we imagine  $RS$ , moving parallel to itself, to approach continually to  $MP$ , it is evident that the point  $T'$  will, at the same time, more and more approach to the point  $T$ ; and consequently, that we may render the line  $T'T$  as small as we please, without affecting the proportion above established. If, then, we neglect the quantity  $T'T$  in the equation just found, there will indeed be an error in the resulting equation  $TP =$

$MP \frac{MZ}{RZ}$ , to which the former will now be reduced: but

this error may be diminished as much as we please, by making  $RS$  approach to  $MP$  as nearly as is necessary; that is, the proportion of the two members of this last equation will differ as little as we please, from the proportion of equality.

In like manner we have  $\frac{MZ}{RZ} = \frac{2y + RZ}{2a - 2x - MZ}$ . (See

Article 3.) And this equation is perfectly accurate, whatever be the position of the point  $R$ ; that is, whatever be the values of  $MZ$  and  $RZ$ . But, the nearer  $RS$  approaches to  $MP$ , the smaller will be the lines  $MZ$  and  $RZ$ ; and hence, if they be neglected in the second member of that equation, the error which will result in the equation  $\frac{MZ}{RZ} = \frac{y}{a - x}$ , will, as in the former case, be thereby rendered as small as we think proper.

This being so, without paying any regard to errors, which I have it always in my power to diminish as much as I please,

I treat the two equations  $TP = MP \frac{MZ}{RZ}$ , and  $\frac{MZ}{RZ} = \frac{y}{a - x}$ , as if both of them were perfectly accurate: substituting, therefore, in the former the value of  $\frac{MZ}{RZ}$ , expressed

in the latter, I have  $TP = MP \frac{y}{a - x}$ ; that is,  $TP = \frac{y^2}{a - x}$ , as before.

This



This result is perfectly just, because it is conformable to that which was obtained by the comparison of the triangles *CPM* and *MPT*; and yet the equations  $TP = MP \frac{MZ}{RZ} = y \frac{MZ}{RZ}$  and  $\frac{MZ}{RZ} = \frac{y}{a-x}$ , are certainly both of them *false*.

For the distance of *RS* from *MP*, was not supposed nothing, or even very small, but equal to a line arbitrarily assumed. It follows, therefore, of necessity, that a mutual compensation of errors took place in the comparison of the two erroneous equations\*.

*Why this Compensation takes place.*

10. Having shown that there exist compensating errors, whence they originated and how they are proved, I shall next proceed to explain them, and to search for a mark by which it may be known that a compensation takes place in calculations similar to the preceding, and the means of producing such compensation in particular cases.

\* This "compensation of errors" the acute Dr. Berkeley, Bishop of Cloyne, in *The Analyst*, published in 1734, laboured to magnify into a serious objection against the differential calculus; not considering that errors which we may make as little as we please, will affect the truth as little as we please. Thus 1.999999, &c. is not, in rigid strictness, equal to 2; but as, by annexing 9's, the error may soon be rendered *incomprehensibly* minute, no one can doubt that its *ultimate ratio* to 2, is a *ratio of equality*. Hence the word *false*, applied by our ingenious author, in this paragraph and elsewhere, to differential equations, is abundantly too strong; and, had he read *The Analyst*, he would certainly have used a less exceptionable term, such as *incorrect*, or *inaccurate*, or *imperfect*, a word which in the sequel he takes in a similar sense. But *false* (*faux, fausse*) signifies not minuteness of error, but a total negation of truth. It is not always easy, indeed, to find words exactly suited to express very abstract ideas. The great Simpson (*Select Exercises*, p. 239.) admits that, had Newton used the word *limiting* instead of *ultimate ratios*, Dr. Berkeley might not have conjured up his "Ghosts of departed Quantities." Yet Mr. Simpson, and all other competent judges, allow that Newton, by representing fluxions not as in the ratio of the whole increments, but in the first ratio of those increments considered as nascent, or in their ultimate ratio considered as evanescent, has effectually anticipated every reasonable objection; and this, no doubt, the learned and ingenious bishop would have confessed, had he given himself time to digest this doctrine, which is partly unfolded in the present tract. W. D.

For this purpose, it will be sufficient to remark, that the errors, included in the two equations  $TP = y \frac{MZ}{RZ}$ , and  $\frac{MZ}{RZ} = \frac{y}{a-x}$ , may be rendered as small as we please; and the same thing would take place if there were any error in the resulting equation  $TP = \frac{y^2}{a-x}$ , and if that error depended on the arbitrary distance of the lines  $MP$  and  $RS$ . But no such error can exist in this last equation; because the point  $M$ , through which the tangent must pass, is given, and not one of the quantities,  $TP$ ,  $a$ ,  $x$ , or  $y$ , is arbitrary.

Hence it follows, that the compensation of errors, which takes place in the equations  $TP = y \frac{MZ}{RZ}$ , and  $\frac{MZ}{RZ} = \frac{y}{a-x}$ , is indicated in the result by the absence of the quantities  $MZ$  and  $RZ$ , which caused those errors. Consequently, after having introduced these last quantities into the calculation, to facilitate the expression of the conditions of the problem, and having treated them, in the equations expressing those conditions, as nothing in comparison of the proposed quantities, we have only to eliminate those quantities out of the equations, in order to free them from error, and to obtain an accurate result.

11. The inventor of this method might therefore have been conducted to his discovery by a very simple train of reasoning. "If, instead of a proposed quantity," might he have said, "I substitute in my calculation a quantity which is not equal to it, some error will certainly result. But if the difference of the quantities in question be arbitrary, and I have it in my power to render that difference as small as I please, the error cannot be dangerous. I may even commit several similar errors at once, without incurring any inconvenience; because I have always in my own power the degree of precision, which I choose to give to my results. Besides, it may happen that those errors will mutually compensate each other; and, if so, my results will become perfectly accurate." But how is such a compensation to be effected in all cases? A little reflection will



will suggest an answer to this question. “In fact,” might the inventor continue, “let me suppose for a moment that the desired compensation has taken place, and let me inquire by what mark it ought to be indicated, in the conclusion of the calculation. Now it must naturally happen, that when the quantities which occasion the errors disappear, the errors themselves will disappear at the same time. For those quantities (such as  $MZ$  and  $RZ$ ) having, by the supposition, arbitrary values, ought no longer to exist in *formulæ*, or results, which are not arbitrary, and which depend, not on the will of the calculator, but on the nature of the things whose relation, expressed by those results, he proposed to investigate. The mark, then, which announces that the desired compensation has taken place, is the absence of the arbitrary quantities which produced the errors; and therefore I have nothing more to do, in order to effect that compensation, than to eliminate those arbitrary quantities.”

12. With a view to fix those ideas the more firmly in the mind, and to give to the principles thence derived the necessary degree of precision and generality, I shall remark, that the quantities which we have occasion to consider in the subject before us, may be distinguished into two classes. The first class consists of quantities which are either given, or determined by the conditions of the problem, such as  $MC$ ,  $MP$ ,  $PT$ ,  $MT$ . The second class is composed of quantities, such as  $RS$ ,  $RT'$ ,  $ST'$ , which depend on the arbitrary position of the point  $R$ , and which, as the point  $R$  approaches to the point  $M$ , do respectively approach their corresponding quantities in the first class. Thus  $MP$ , for example, is the limit of  $RS$ , that is, the fixed term to which it continually approaches, or, if you will, its last or *ultimate value*. In like manner,  $MT$  is the limit or ultimate value of  $RT'$ , and  $PT$  that of  $ST'$ . For the same reason, it is clear that the limits or ultimate values of  $MZ$ ,  $RZ$ ,  $MR$ ,  $T'T$ , are, every one of them, 0. In fine, it is also evident that the *ultimate* ratio of  $RS$  to  $MP$ , (that is, the ultimate value of  $\frac{RS}{MP}$ ), is the ratio of equality, and such is the ratio of  $RT'$  to  $MT$ , of  $ST'$  to  $PT$ , or, in a word, such is that of any other quantity to its limit.

13. To enable us to extend these remarks to other problems of the same kind, let us now put the case, That any system whatever of quantities is proposed, in order that the relations which exist between them may be investigated\*.

14. And first, I shall comprehend under the name of *assigned* quantities (*quantités designées*) not only all the quantities proposed in the very enunciation of a problem, but also all those which depend on those quantities, that is, which are functions of those quantities, and of none else.

15. On the other hand, I shall give the names of *unassigned*, or *auxiliary* quantities, to all those which make no part of the system of assigned quantities, and which consequently do not essentially enter into the calculation, but are introduced solely to facilitate the comparison of proposed quantities.

Thus, in the preceding example,  $MP$ ,  $MC$ ,  $MT$ ,  $DP$ , &c. are *assigned* quantities; because they depend only on the position of the point  $M$ , to which the tangent is to be drawn. But  $RS$ , and all its dependent lines,  $MZ$ ,  $RZ$ ,  $T'T$ ,  $T'P$ , &c. are *auxiliary* quantities; because we should not have thought of drawing them, except their assistance had been wanted in the solution of the question, the object of which is to discover the proportion, or relation, of  $MP$  to  $MT$ .

\* I here suppose that, in any problem proposed, the relations which exist between such or such proposed quantities has been previously discovered. If, for example, the problem be, to find a curve which has a certain determinate property, I suppose that the relation between an ordinate of that curve and the corresponding abscissa is already known. In like manner, if it were required to draw a tangent to any indeterminate point of that curve, I begin by arbitrarily fixing on the point to which I wish to draw the tangent, and I limit the problem to the investigation of the relation which subsists, for instance, between the subtangent and the abscissa, or between the ordinate and the corresponding subnormal at that point. But if it should be asked how I apply the definition of *infinity* to such questions as these: *Is matter divisible ad infinitum? Is the space in which all created beings exist infinite?* I say, if such questions should be put to me, I should answer, that my definition is restricted to mathematical infinity, and that it can only be applied to problems whose object is the discovery of the relations which exist between different mathematical quantities, and that the metaphysical questions just stated can by no means be referred to the theory, of which I here propose to establish the principles.

Hence



Hence it evidently follows, that into all unassigned quantities there necessarily enters something arbitrary; for, if this were not the case, their value would then be assigned, by the very conditions of the problem, and consequently they would wholly depend on proposed quantities; which is contrary to the supposition.

16. Since in the mathematics, two lines, two surfaces, two solids, in short two quantities of any kind whatever, are supposed continually to approach each other by insensible degrees; so that their proportion, or the quotient representing that proportion, differs less and less, and as little as we please, from unity, we say, that the last, or ultimate ratio of those two quantities is a ratio of equality.

17. If one of the quantities be assigned, and the other auxiliary, the first is called the *limit*, or *ultimate value* of the second. Thus a limit is nothing else than an assigned quantity, to which an auxiliary quantity is supposed continually to approach, in such a manner that their difference may be rendered as small as we please, and that their ultimate ratio may be a ratio of equality.

Thus auxiliary quantities alone can be properly said to have limits; for assigned quantities, being supposed constant and unchangeable, and to be themselves the terms, or ultimate values of auxiliary quantities, cannot, in strictness, be said to have any limits; unless we choose to say, that every assigned quantity is its own limit, a liberty of speech which cannot be refused us; since the ultimate value of any determinate quantity whatever can be nothing else than that quantity itself.

18. Hence, in general, we give the names of ultimate values and ultimate ratios of quantities, to the values and ratios which are, in fact, the last, or ultimate, ones assigned to those quantities and their relations, by the law of continuity, when each of them is supposed to approach, by continued and insensible degrees, to its corresponding assigned quantity.

19. We generally give the name of an *Infinitely small* quantity, to the difference between any auxiliary quantity whatever and its limit. Thus, for instance,  $RZ$ , which is

the difference between  $RS$  and  $MP$ , is what we call an infinitely small quantity.

20. On the other hand, we call every quantity *Infinitely great*, or simply *Infinite*, which is equal to unity divided by an infinitely small quantity: consequently the quantity  $\frac{1}{RZ}$  or  $\frac{1}{RS-MP}$  is infinite, or infinitely great.

But, since  $MP$  is the limit, or ultimate value, of  $RS$ , it is evident that 0 is the ultimate value of  $RZ$ , or  $RS-MP$ , and that  $\frac{1}{0}$  is the ultimate value of  $\frac{1}{RZ}$ .

21. Thus we may affirm, in general, that *An infinitely small quantity is a quantity whose limit is 0*; and on the contrary, that *An infinitely great, or simply an infinite, quantity is a quantity whose limit is  $\frac{1}{0}$* .

22. Under the name of *Infinitesimal quantities*, we comprehend quantities which are infinite, or infinitely great, as well as those which are infinitely small. All other quantities are called *finite*.

23. To say, in conformity with common usage, that Infinity is that which has no bounds, or is that which is without limit, or is that whose limit does not exist, is a mode of expression which conveys a simple idea of what is meant, and which is by no means destitute of propriety; because in fact, the limit of one kind of infinitesimal quantities is 0, and that of the other  $\frac{1}{0}$ ; and 0 and  $\frac{1}{0}$  are not real quantities\*.

24. But,

\* This kind of notation, which is in general use, is very apt and significant. But we are under no necessity of making 0 always represent *absolute nothing*. We are at liberty, when our subject requires it, to make 0 stand for *relative nothing*, or that which may be considered as nothing relatively to some other quantity. Thus any very small fraction .000000000001, (prefixing cyphers at pleasure) though not absolutely nothing, may be safely considered as nothing, *relatively* to, or compared with 1, and, *à fortiori*, with any other whole number, and accordingly may be represented by 0. And, if so,  $\frac{1}{.000000000001}$ , or 100000000000 (annexing cyphers at pleasure) though by no means absolutely infinite, may be accounted infinite relatively to 1, or any moderate given number, and accordingly may be represented by  $\frac{1}{0}$ .—Thus the diameter of the earth



24. But, from  $0$  and  $\frac{1}{0}$  being the limits of infinitesimal quantities, it by no means follows that these quantities themselves are chimerical entities. On the contrary, it appears from the definition (article 19) that an infinitely small quantity is the difference between two very efficient quantities, namely, between any auxiliary quantity whatsoever and its limit.

25. Hence it follows farther, that every infinitely small quantity may be considered as the difference between two auxiliary quantities, which have the same third assigned quantity for their limit. For, let  $X$  and  $Y$  be two different auxiliary quantities, whose limit is the same third quantity  $A$ . I say, that  $X - Y$  is an infinitely small quantity. For, since the limit, or ultimate value of  $X$  is  $A$ , and that of  $Y$  is also  $A$ , it follows that the ultimate value of  $X - Y$  will be  $A - A$ , or  $0$ . Therefore the limit of  $A + (X - Y)$  is  $A$ ;

earth may be reckoned *very small*, compared with the distance of the sun, and as *relative 0*, compared with that of any fixed star; and conversely, the distance of the sun is *very great*, and that of a fixed star  $\frac{1}{0}$ , or infinite, compared with the earth's diameter.—Again; supposing a rectangle to be continually diminished by the parallel motion of one of its sides, the law of continuity tells us that it will come to  $0$ , or that the moving side will arrive at the fixed side of the rectangle; so that  $0$  here is not absolute nothing, but it is a line, which however may be looked upon as  $0$ , if compared with the rectangle; and the rectangle as  $\frac{1}{0}$ , or infinite, if compared with the line. In like manner, a point and a rectangle are as  $0$ , or nothing, when respectively compared with a line and a parallelopiped; and these last as  $\frac{1}{0}$ , or infinite, when respectively compared with the two first. Thus we see that the sense of the  $0$  is always to be determined by the subject into which it is introduced. See Emerson's P. S. to the 2d edition of his Algebra, where he replies to the Reviewers, who had misunderstood the very elegant solutions of his 73d and 74th problems.

The 1st, 3d, 6th, and 7th corollaries to the former of these problems will be useful in this place.

“ Cor. 1. If  $0$  multiply any finite quantity, the product will be nothing.

“ Cor. 3. If a finite quantity be divided by  $0$ , the quotient is infinite.

“ Cor. 6. Adding or subtracting any finite quantities to or from an infinite quantity, makes no alteration.

“ Cor. 7. Therefore, in any equation where are some quantities infinitely less than others, they may be thrown out of the equation.”

W. D.  
and

and consequently we may consider  $X - Y$  as the difference between an auxiliary quantity  $A + (X - Y)$  and its limit  $A$ , and this difference (by article 19.) is an infinitely small quantity. It may therefore be affirmed, in general, that *An infinitely small quantity is the difference between two auxiliary quantities which have the same limit.*

26. Two quantities cannot have the same third quantity for their limit, unless the ultimate ratio between those two quantities themselves be a ratio of equality. For, since, by the supposition, the limit, or ultimate value of  $\frac{X}{A}$  is 1, and that of  $\frac{Y}{A}$  is also 1, it is evident that the limit, or ultimate

value of  $\frac{\left(\frac{X}{A}\right)}{\left(\frac{Y}{A}\right)}$  is likewise unity. Now  $\frac{\left(\frac{X}{A}\right)}{\left(\frac{Y}{A}\right)} =$

$\frac{X}{Y}$ ; and therefore the limit, or ultimate value of  $\frac{X}{Y}$  is 1; that is, the ultimate ratio of  $X$  to  $Y$  is a ratio of equality. It may therefore be affirmed, in general, That *An infinitely small quantity is the ratio which the difference of two quantities, ultimately in the ratio of equality, has for each of these two quantities.*

27. In fine, it may evidently be farther affirmed, That *An infinitely small quantity is an unassigned quantity, to which is at first attributed any arbitrary value whatever, and which is afterwards supposed to decrease insensibly till it arrive at 0, or zero.* Thus, in general, when it is said, *Let Z, for example, be an infinitely small quantity,* it is precisely tantamount to saying, *Let Z be any arbitrary quantity whatever,* (and consequently auxiliary, for assigned quantities cannot be arbitrary) and let us afterwards suppose that quantity ( $Z$ ) to decrease continually till it come to 0, or zero\*.

28. A quantity is said to be infinitely small *relatively* to another quantity, when the ratio of the first to the second is

\* In the 25th, 26th and 27th articles, the author gives us different ideas of an infinitely small quantity; but which, if duly considered, will be found to amount nearly to the same thing. W. D.



an infinitely small quantity; and, reciprocally, the second is said to be infinite or infinitely great, *relatively* to the first.

29. Two quantities are said to differ infinitely little, or to be infinitely little different, from one another, when the ratio of the one to the other differs from unity only by an infinitely small quantity; so that their ultimate ratio is a ratio of equality, and such evidently are *RS* and *MP*.

30. The *Infinitesimal Calculus* may be defined to be the art of discovering, by the help of quantities called Infinitesimals, the proportions or relations, whatever they may be, which subsist between the different parts of any system of quantities proposed.

These infinitesimal quantities being nothing more than auxiliary quantities, introduced into the calculation only to facilitate the expression of the conditions of any proposed problem, it is evident that they must necessarily be eliminated out of the calculation, in order to obtain the desired result, namely, the relations sought. Thus we may in some respects affirm, That every Infinitesimal Calculation is an *unfinished* calculation; for in fact, as soon as the auxiliary quantities, which are not essential, come to be eliminated, the calculation ceases to be infinitesimal, and in every thing resembles an ordinary algebraic result\*.

To complete the explanation of the principal terms which relate to the theory of Infinites in general, it remains for me to state what I understand by an imperfect equation.

31. I call every equation *An imperfect equation*, whose two sides are unequal quantities, the difference however between them being infinitely small; or, which comes to the same thing, Every equation is imperfect, the two sides of which, though unequal, have for their ultimate ratio, the ratio of equality.

Thus, for example, the *false* equations,  $TP = y \frac{MZ}{RZ}$  and  $\frac{MZ}{RZ} = \frac{y}{a-x}$  (see article 9.) are what I call imperfect

\* Every one knows, indeed, that a calculation, in which Infinitesimal quantities have any place, is not supposed to be finished, and that we never think the result accurate till all the infinitesimal quantities are entirely eliminated.

equations; because the quantities, neglected in the accurate equations, whence they are derived, are infinitely small quantities. On the theory, therefore, of this kind of equations is founded the solution of the question before handled, and that of all questions of that sort. For this reason it is, that I shall proceed to investigate the principles of the theory of imperfect equations, which is the foundation of the Infinitesimal Calculus, or rather, which is itself that calculus.

[To be continued.]

V. *Account of C. F. DAMBERGER'S Travels through the interior Parts of Africa, from the Cape of Good Hope to Morocco\*.*

THIS adventurous traveller, a native of Germany, and brought up to the business of a carpenter, enlisted at Amsterdam, in the year 1781, as a soldier in the service of the Dutch East India Company, with whom he entered into an engagement for seven years. On the 21st of June he sailed from the Texel, and on the 21st of September arrived at the Cape of Good Hope, where he was conveyed to the hospital along with eighty-four other sick, who had come out in the same vessel. After being a month in the hospital, he was relieved from military duty, by being taken into the service of Mr. Brand, the postmaster at the Cape and president of the council, who sent him to False Bay, where he remained a whole year, employed in various labours, such as loading and unloading ships, cutting down wood in the mountains, and other things of the like kind.

Soon after he was promoted by his master to be his house-steward, and in this situation discharged his duty for some time to the complete satisfaction of his employer, who afterwards carried him back with him to the Cape Town; but having quarrelled with his mistress, he found himself exposed to so much ill-treatment, that he resolved to desert, and to make his way to Europe by land. In order that he might be better able to carry this plan into execution, he requested his

\* From C. F. Damberger's *Landreise in das innere von Afrika*, in den Jahren 1781 bis 1797. Leipzig, 1801.



master to send him back to False Bay, where he expected he should have an opportunity of effecting his escape with the less danger of detection. This request was granted, and he set out for the Bay on the 14th of November 1783, after having put his box into the Company's waggon, received twelve months pay due to him, and disposed of various articles which he considered as entirely useless for his intended expedition.

Among the Dutch troops at the Meisenberge was a corporal named Martens, a native of Hanover, who having accompanied Colonel Gordon on a journey into the interior part of the country as far as Caffraria, had constructed a map of the districts through which he had passed and presented it to the Company, under an idea that he should be rewarded for his trouble; but instead of a reward, he was enjoined, under the pain of severe punishment, never to attempt any thing of the like kind in future. This man however was so fond of these pursuits, that he still amused himself with making charts, and our traveller after some difficulty prevailed on him to allow him to copy some of them in order to assist him in his enterprise.

On the 24th of December, every thing being ready for his journey, he obtained leave of absence from the sergeant on command under pretence of going to the Cape Town, but proceeded towards Stielenbusch, so called from a governor of the name of Van Stiel, who formerly presided over the Dutch settlements at the Cape, where he arrived next evening. Being acquainted with the bailiff of this farm, who had often seen him at Mr. Brand's house, he accosted him with confidence, and was received in a very friendly manner, especially as he told him that he was going to his master's plantation, to see that every thing was properly conducted.

At this place Damberger purchased a new fowling-piece, and on the 26th proceeded to the farm of another acquaintance, named Munch, from whom he obtained two pounds of powder and thirty balls. On the 27th he came in sight of his master's farm, but did not approach it till the evening, lest he should be observed by the bailiff. As soon as it was dark he went privately into the apartment inhabited by the slaves,

whom he requested not to say any thing of his arrival, and, after partaking of some refreshment, retired to rest, but with a mind so agitated that he enjoyed very little sleep. Next morning at day-break he continued his journey, and on the 19th of February reached Blettenberg's Bay, where he purchased several things necessary for his intended expedition, such as a calabash to hold water, a few more pounds of powder, a flint and steel, matches, &c. During the few days that he remained here, he took up his lodging in the house of a person named Vogtmeyer, who was not then at home; but he was kindly entertained by his wife, and he embraced this opportunity of obtaining from the Hottentots in the family every information possible respecting the route he intended to pursue. The country around this place has a beautiful appearance, and is exceedingly fertile; it abounds with timber of every kind, and has a good supply of excellent water. When Vogtmeyer returned, which was on the 25th of February, he received our traveller with great friendship, but at the same time told him that he knew he was a deserter, and that he would send him back to the Cape, where he was sure he should be well rewarded by his master. He however entertained no such design; for, after being informed of Damberger's real views, he furnished him with a knapsack made of a calf's skin, a hand-bill, with several other small articles, and wished him a good journey.

Our traveller, who had now got to the distance of 74 miles from the Cape, again set out on the 26th of February, and, pursuing his way towards Caffraria, arrived next day at the first Hottentot kraal he had seen, which consisted of about twenty huts. In this kraal he remained nine days, at the end of which time he had an opportunity of continuing his journey in company with some Hottentots of another kraal, who had come hither to procure sheep. Having proceeded two miles with great difficulty through the long grass, which rendered their progress exceedingly tedious and tiresome, they halted for the night on the banks of the Silver River; but were unable to sleep for fear of the wolves, which, attracted by the sheep, kept prowling around them; as they were afraid of kindling fires to frighten them away, lest they  
should



should set fire to the grafs, which was exceedingly long and almost dry.

In the morning they reached the fummit of the Milk Mountains, where they found good pasture; they therefore fat down to breakfast, and turned their fheep loofe to feed; but fcarcely had they put a morfel to their mouth, when they efpied ten men rapidly advancing towards them. Our traveller having foon learned from his companions that thefe ftrangers were Boshmen, who no doubt would attempt to plunder them of their fheep, he endeavoured to infpire them with courage, and, immediately loading his piece, fired it at the enemy, one of whom fell; the ref then difcharged their affagays, and betook themfelves to flight.

Next morning our traveller arrived at the kraal where his companions refided. As he was much fatigued, having fpent feveral nights without the leaft repofe, he threw himfelf down in one of the huts and enjoyed a found fleep. When he awoke the Hottentots brought him fome milk and roaft mutton; and whilft he was partaking of this repaft the chief of the horde entered the hut in his drefs of ceremony, and, bowing feveral times before him, thanked him for the affiftance he had given to his countrymen. The chief then told him that thefe Boshmen, a few days before, had ftofen from the kraal thirty fheep. Our traveller, in confequence of this information, inquired what might be the ftrength of the horde, and where they refided. Being informed that they confifted of about thirty men, and that they refided at a place in the mountains a few miles diftant, where, to guard againft the danger of an attack, they had fortified themfelves behind a kind of abbatis, and were furnifhed with a great number of ftones, he told the Hottentots that he thought it not impoffible to drive them from their lurking-place, and offered to lead the attack, if they would promife to fupport him. The chief liftened to this propofal with great pleafure, as it held forth hopes of recovering the ftofen fheep; but he fstarted fome objections as to the practicability of carrying it into execution. It was however at laft agreed that the attempt fhould be made, and Damberger, after giving proper inftructions to the Hottentots, defired each to furnifh himfelf

with a stout cudgel, a bundle of dry grafs, and a little dry wood. As soon as it grew dark, they proceeded in great silence towards the mountain, where, as soon as they arrived, our traveller divided the whole troop into two bodies, one under his own command, and the other under that of the chief. They then rushed towards the first fence, which being instantly set on fire, the flames were soon spread by the wind, and in a little while the whole mountain seemed to form one general conflagration. They now proceeded to a place at the bottom of the mountain, by which it was supposed the enemy would be obliged to make their escape. Here they had not long waited, when they heard a dreadful howling, and saw several of the Boshmen half scorched rushing through the flames. To terrify them the more, and at the same time to give a more general signal to the Hottentots to attack them, our traveller discharged his piece among the flying enemy, who were attacked with such fury by his Hottentot allies, that this whole horde of plunderers were extirpated. Next morning they not only recovered some of the stolen sheep, but found several more which had belonged to the enemy. When the conquerors returned to the kraal, they were received with great joy. As our traveller found himself much fatigued by the expedition, he retired to enjoy some rest in the chief's hut, but the Hottentots spent the whole night in rejoicing on account of their victory.

Damberger continued two weeks among these people, who treated him with great respect; but as it was then the most favourable period for travelling, he resolved to proceed and to take advantage of the winter season, as it is called, which takes place in April, and which however is distinguished only by the weather being rainy and the air cooler.

In consequence of this resolution he left the kraal on the 25th of March, and directed his course eastwards towards Fish River, but with some caution to avoid falling in with any of the Dutch plantations, of which there were several in the neighbouring districts. After passing another kraal he took up his lodging for the night in a third, which was the last within the jurisdiction of the Dutch East India Company. It was situated on the Fish River, at a place where it divides  
itself



itself into two arms, one of which flows between the mountains and the Dutch colonies, and the other into the Silver River already mentioned.

The author in speaking of the Hottentots says, “ Mr. Vaillant has given a very ample description of the manners and customs of these people, but he is not always correct.

“ The chief of a horde bears the title of *Montur*. This office however is not hereditary, and the persons chosen to fill it are those who have distinguished themselves by some heroic action, such as killing some savage animal, or obtaining a victory over the enemy. The chief of each kraal, whether it lies within the jurisdiction of the Dutch or not, receives from the Company a large cane, having at the top a silver knob, inscribed with the Company's arms, and which is borne by the chief on solemn occasions. Each Hottentot also receives annually two pounds of tobacco and two bottles of brandy; but, in return, must endeavour to learn in some measure to understand the Dutch language. Those who reside among the planters learn it with great ease; but those who live at a distance must embrace some other opportunity of learning it: and when the *Monturs* go to the Cape Town, they must always carry with them some men belonging to their horde, to whom various occupations, such as cutting wood, &c. are assigned in the castle, that they may become familiar with the language by frequenting the company of the soldiers, and the inhabitants. When the *Montur* goes to the Cape the next year, he carries with him some more of his countrymen, and those who came the preceding year return. These Hottentots are perhaps the laziest people in the world; but in many parts of their country they are destitute of water. They leave the most fertile fields uncultivated, and only plant here and there a little Turkish corn. Their chief occupations are the rearing of cattle and hunting, but they pursue the latter only when compelled by necessity and the want of food. They pay little attention to fishing, though they have very good opportunities, especially in winter, of catching fish without great exertion. There have been instances of their enduring hunger for several days rather than give

give themselves the least trouble to search for food. The Company have frequently offered to supply them with nets, corn, and other articles, and to assign lands to them nearer the Cape, if they would only become a little more active and industrious—but without the least effect; as they believe that in this case they should be exposed to more bodily labour. They prefer living in poverty in the midst of the deserts, in a state little superior to that of their cattle; and are so timid as to suffer themselves to be driven sometimes towards the South by the Caffres, and sometimes towards the East by the Boshmen. If they could only assume a little courage, they would soon make their enemies sensible of their power; but they have very little care for the future.

“ The women are very active in the management of their domestic economy, but have a considerable degree of vanity, for they employ a great deal of their time in ornamenting their persons. To a mother a child is a great treasure, and the husband dare never presume to take it into his hands as long as it is at the breast, though in other respects he treats his wife as his slave. This right has been claimed by the mothers in consequence of the cruelties formerly practised by the fathers; for, when the Portuguese first visited this country, many of the men sold their children to them for brandy or tobacco; this incensed the mothers, and therefore they assumed the above privilege. A child is suckled for four months, during which time the mother in the day carries it about on her back wrapped up in a sheep’s skin, but in the night places it in her bosom; after that period it is suffered to lie on a mat spread out on the grass, and has the same food as the rest of the family.

“ I was much pleased with their treatment of the dead. They never bury them until every mean possible has been tried to recall them to life. Those who die of chronic or lingering diseases, they shake and beat on the ribs with their fists; if they exhibit no signs of life, they are interred the same evening, but not until similar experiments have been tried to revive them. On young persons who die suddenly other methods are tried: the soles of their feet are pricked  
with



with affagays \*, pieces of red hot iron are applied to their nose, and their bodies are well rubbed; but if no signs of life appear, they are committed to the earth next evening. The corpse is immediately carried out from the hut, and watched by some of the relations, who do the same thing for twenty-eight days after it has been buried, in order to prevent it from being torn up by wild beasts.

Our traveller resumed his journey on the 25th, directing his course towards Fish River, and on the 29th entered Caffraria, where at first he had to ascend steep mountains, but towards evening descended into the level country. After passing through several hordes of Caffres, by whom he was in general well received, he arrived at a kraal consisting of 127 huts, and containing 1400 inhabitants. As he was here treated with great friendship, he remained some weeks; and having heard from the natives, among whom he saw several European articles, that a ship had been stranded on the coast, at a place lying at the distance of two days journey, he resolved to go thither, and prevailed on some of the Caffres to accompany him. They accordingly set out on the 27th of April, and next evening reached the remains of the vessel, which were lying in a bay between Fish River and the river St. Lucia. Here they found several half-putrid bodies, most of them pierced with wounds, scattered about on the shore, and intermixed with casks, chests, bales of cotton, and other articles, entirely spoilt by the sea water and the sun. What chiefly attracted the notice of the Caffres was the iron work belonging to the ship, of which they collected as much as they could. After burying the bodies †, and picking up various articles, among which was a pocket compass, they set out to return. The compass, which was not injured, fell

\* The points of their affagays are not poisoned except when they go out to attack their enemies.

† This ship had been lost in consequence of a mutiny which broke out among the crew, who first murdered their officers, and then ran the vessel on shore. The crew then landed, and wandered about in the country, where they either fell a sacrifice to the climate or were massacred by the Caffres. Of the whole ship's company four only escaped, and of these four two died of their wounds. The other two made their way to the Cape.

to the lot of our traveller, who considered it as an acquisition of no small importance.

On the 20th of May, Damberger took leave of these friendly Caffres, and, after encountering considerable dangers and difficulties between that period and the 11th of July, directed his course eastwards, with a view of proceeding to Egypt. Towards evening he discovered some huts at a distance, but was not able to reach them; he therefore lay down on the grass with his head on his bundle to enjoy a little rest, but was soon roused by four Caffres, who conducted him to their huts. Here he was robbed of his fowling-piece and bundle, but they were both afterwards restored. He was attacked also with a violent pain in his bowels; but on making known his complaint to one of the Caffres, he gave him some dried leaves to chew, which acted as a strong emetic, and soon afforded him relief.

Among the next horde whom he visited he was more unfortunate, being robbed both of his bill and his fowling-piece; the former he recovered, but the latter was broken to pieces by the Caffres in order that they might convert the iron part of it into points for their assagays.

Soon after leaving this horde he was obliged to pass the night in the open air on the banks of a rivulet, where he made a large fire to frighten away the wild animals, but was unable to sleep a moment on account of the multitude of snakes which swarmed around him; and towards morning he saw hundreds of large baboons. The latter had taken up their station on all the neighbouring trees, and did not seem in the least shy or afraid.

Of these people the author says, "This nation, as well as several more of the African tribes, are accused of indolence, but I am convinced that the industrious Europeans, if transported hither, would be equally inactive. The heat oppresses the labourer too much, and exhausts the powers of the body; to this it may be added, that most of these tribes are destitute of the necessary implements, such as spades, hoes, &c. and the ground is so hard that, before any other tool can be used, it must be cut up with a sort of pickaxe. The cultivation of the few fields, which are sown here with Turkish wheat, consumes



consumes a great deal of time and requires much labour. I observed that two stout active men could till in a day no more than six feet square. The land, after being sown, was covered, to the height of two inches, with sand, in order that the excessive heat might not too soon dry up the moisture, and to prevent the ground from cracking by the drought and the rain. They derive more advantage, without much labour, from rearing of cattle, and from hunting and fishing. This nation, indeed, have a strong propensity to stealing, but they never commit murder; and much less do they devour human flesh. Several Europeans \*, however, who have visited these people, assert that they eat human flesh with great avidity; but this is absolutely false. If a traveller understand their language he has nothing to fear, especially if badly clothed and in possession of no articles of value. If they take any thing from him, he may rest assured that they will give him something of theirs in return if he asks for it.

“The dead are interred at some distance from the kraal, and the relations kindle a large fire near the spot, which they maintain three days, to prevent the savage animals, that might be attracted by the smell, from coming to tear up the body. Few persons die in the kraals, as the sick are removed to huts situated at a distance; for they believe that every disease is infectious, and that it is their duty to guard against the contagion.”

After a stay of three weeks among these people, whom our traveller calls the Jamatians, he continued his journey, and, crossing the Makumbo river, entered the territories of the Muhotians. Here he was at first treated with great harshness and severity, but he still found friends among the female sex. While he remained with this tribe he was conducted to a place where he found the bodies of five white men covered with wounds, and apparently pierced with assegays: on their right arms he observed the figure of a crucifix, with the letters H, I, E, M, and the date 1779.

On the second of October he arrived among another tribe called the Kamtarians, who inhabit a district situated on the river Tambo, and who live chiefly by hunting and the rear-

\* Kolbe, Sparmann, Patterfon, &c.

ing of cattle. The men in general are of low stature, and have short woolly hair. These people were of a darker complexion than the other tribes he had quitted. They consisted of six or seven thousand, of both sexes, capable of bearing arms. The women were of larger size than the men, and exceedingly bold and courageous. The greater part of them had been carried away by force from other nations, or had been taken prisoners in war and retained by the men. These people never rear their female children, but destroy them as soon as they are born. There are no priests among them, though there is reason to think that they are not destitute of religion. The oldest inhabitant in each village acts generally in the capacity of chief and judge. Polygamy is allowed among this tribe; and every woman not pregnant by her husband is at liberty to cohabit with any other man whom she chooses, and if she bring forth a boy to him, she may become his wife.

These people are exceedingly superstitious, and, when the least misfortune befalls them, shut themselves up in their huts, where they remain totally inactive for three or four days. When any one of their nation has been wounded or killed by wild beasts while hunting, they say "he offended the great god;" but if such an event take place in the night-time, and particularly by moon-light, they say, "he offended the little god," and on that account was punished. If a woman die in childbed, which, however, is seldom the case, she is buried in a particular spot, and her family must remain six moons, at some distance from the village, in huts set apart for that purpose, without having any intercourse with the rest of the tribe. If a woman bring forth a male child during cloudy weather, or when the moon does not shine, it is concluded that his father has offended the gods, and that the boy is not worthy of being a respectable member of society. When he grows up, therefore, he is employed in the most menial offices, such as cutting wood, guarding the cattle, and other services of the like kind. Neither circumcision, nor any similar practice, is known in this country.

If a woman be delivered of a boy at a fortunate period, the father kills a goat, and gives an entertainment to the friends  
of



of his family. The dead are generally buried by their relations under large trees. During the time of the funeral a large fire is kindled, in which the effects of the deceased are burnt, and the ashes are thrown into the grave. The fire is kept burning till the next full moon. Whoever steals a married or unmarried female, may keep her as his wife; but, if he does not choose to do so, he may sell her, and in return generally receives a sheep, or a greater or less number of assagays, according to her beauty. These people are kind and hospitable to strangers.

Damberger left the Kamtarians on the 7th of October, and on the 30th, after passing through part of the kingdom of Biri, arrived in the territories of that of Mataman. The sovereign of this country rules with unlimited power, and is stiled *Sobaawoia*, that is, Chosen by the gods. The crown is hereditary to females as well as males. When one of the former succeeds, she may choose from her subjects a husband to participate with her in the government. The person so chosen, however, must be first examined by the oldest part of the community, in order that it may be known whether he possesses the talents necessary for the intended dignity. The king here acts in the capacity of high-priest, prophet, and tutor to the youth. His decisions are respected even when he judges wrong. He possesses the exclusive privilege of having a plurality of wives, and must install the inferior judges, priests, &c. He never takes the field against the enemy, but delegates the command of the army to another. The soldiers are brave, and exceedingly expert in managing their bows and arrows. They are said to amount to about 30,000. There are three large cities in the kingdom, the capital of which, called Seenhofa, lies at the distance of two days journey. This city, the residence of the king, our traveller resolved to visit; and, being furnished with a person to show him the way, soon reached the place. On his arrival he repaired to the *Mobwoia*\*, who gave him some corn and four milk, and conducted him into his hut, where he was

\* This is the name given to the governors appointed in each town by the king, and who at the same time act as priests.

allowed to pass the night. Next morning the Mohwoia carried him through a long street, bordered on each side with huts, to a kind of green, where he was introduced to the king, whom he found standing amidst a circle of his attendants, and who appeared to be about forty years of age. The day before he had taken four florins from his vest\*, in order that he might make an offer of them to the sovereign. This he accordingly did, and requested his majesty's protection and a supply of food. The prince, after looking some time at the money, ordered one of his wives to fetch some milk and meal, which being mixed together and stirred round, our traveller sat down on the ground and made a hearty meal. He then accompanied the king into his hut, where a mat was brought for him to sit upon. The king asked him for some more money, and told him that in return he would make him a *kahseeto* (servant). Damberger protested that he had none; but, as he was desirous of becoming acquainted with the manners and customs of the country, he informed the king that he gratefully accepted his offer. Observing, however, some days after, that he was considered in the light of a common slave, he resolved to make his escape the first opportunity he could find for that purpose.

On the 29th of November, having been obliged to accompany the king his master on a hunting excursion, for the purpose of carrying a calabash filled with water, a leather bag containing millet, and a javelin, in passing through a wood interspersed with small hills he pretended to be suddenly taken ill with a pain in his bowels, and immediately sat down on one of these eminences. The king, not suspecting any deception, gave him permission to remain till he should call for him, and, taking the javelin from him, proceeded forwards; but as soon as his master was out of sight, our traveller set off as fast as he could, pursuing a northern direction with a view of reaching the mountains. In crossing a river he stumbled against a stone and hurt his foot, by which means his progress was considerably retarded; and falling in soon after with some of

\* Some time before this period the rest of his clothes had become so ragged and filthy that he was obliged to burn them.



the natives, they compelled him to take upon his back a young wolf they had found, and to return with them to a village which he had left only a few hours before. Here he was detained eight days, during which he suffered considerably from the wound in his foot; but being at length suffered to depart, and proceeding towards the territories of the Seege-rians, he arrived at a village called Mukosa, situated on an eminence.

[To be continued.]

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VI. *Experiments on the Solar and on the Terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different.* By WILLIAM HERSCHEL, LL.D. F.R.S.

[Continued from Page 134.]

14th Experiment. *Refraction of the Heat of a Chimney-Fire.*

I PLACED Mr. Dollond's lens before the clear fire of a large grate\*. Its distance from the bars of the grate was three feet; and in the secondary focus of it was placed the thermometer No. 1. No. 4 was stationed, by way of standard, at  $2\frac{1}{2}$  inches from the former, and at an equal distance from the fire. Before the thermometers was a slip of mahogany, which had three holes in it,  $\frac{8}{10}$  of an inch in diameter each. Behind the centre of the first hole,  $\frac{3}{8}$  of an inch from the back, was placed the thermometer No. 1; and between the second and third hole, guarded from the direct rays of the fire by the partition, at the same distance from the back, was put No. 4. Things being thus arranged, the situation of the apparatus which carried the thermometers, and that where the lens was fixed, were marked. Then the thermometers, having been taken away to be cooled, were restored to their places again, and their progress marked as follows:

\* See Plate VIII. fig. 3.

	No. 1. Burning lens.	No. 4. Screened.	} Here, in nine minutes, the rays coming from the fire, through the burning glass, gave $9\frac{3}{4}$ degrees of heat more to the thermometer No. 1, than No. 4, from change of temperature, had received behind the screen. Now, to determine whether this was owing merely to a transmission of heat through the glass, or to a condensation of the rays, by the refraction of the burning lens, I took away the lens, as soon as the last observation of the thermometers was written down, and continued to take down their progress as follows:
0'	58	58	
$1\frac{1}{2}$	65	60	
3	68	61	
5	70	$61\frac{1}{2}$	
7	$71\frac{1}{4}$	$61\frac{3}{4}$	
9	$71\frac{1}{2}$	$61\frac{3}{4}$	

mine whether this was owing merely to a transmission of heat through the glass, or to a condensation of the rays, by the refraction of the burning lens, I took away the lens, as soon as the last observation of the thermometers was written down, and continued to take down their progress as follows:

	No. 1.	No. 4.	} Here the direct rays of the fire, we see, could not keep up the thermometer No. 1; which lost $2\frac{1}{4}$ degrees of heat, notwithstanding the lens intercepted no longer any of them. I now restored the burning glass, and continued.
$9\frac{1}{2}$	$71\frac{1}{2}$	$61\frac{3}{4}$	
11	$70\frac{1}{2}$	$61\frac{3}{4}$	
12	$70\frac{1}{4}$	$61\frac{3}{4}$	
—	$69\frac{1}{2}$	$61\frac{3}{4}$	
$14\frac{1}{2}$	$69\frac{1}{4}$	$61\frac{3}{4}$	

any of them. I now restored the burning glass, and continued.

	No. 1.	No. 4.	} Here again, the lens acted as a condenser of heat, and gave $1\frac{3}{4}$ degrees of it to the thermometer No. 1. I now once more took away the lens, and continued the experiment.
15'	$69\frac{1}{4}$	$61\frac{3}{4}$	
16	$69\frac{1}{2}$	$61\frac{3}{4}$	
17	70	$61\frac{3}{4}$	
20	$70\frac{3}{4}$	$61\frac{3}{4}$	
25	71	$61\frac{3}{4}$	

the experiment.

	No. 1.	No. 4.	} This again confirms the same by a loss of 3 degrees of heat. I restored the lens once more, and had as follows:
$25\frac{1}{2}$	71	$61\frac{3}{4}$	
31	68	$61\frac{3}{4}$	

And here the thermometer received  $1\frac{1}{2}$  degree of heat again; so that, in the course of 35 minutes, the thermometer No. 1 was alternately raised and depressed five times, by rays which came from the chimney fire, and were subject to laws of refraction, not sensibly different from those which affect light.

#### 15th Experiment. *Refraction of the Heat of red-hot Iron.*

I caused a lump of iron to be forged into a cylinder of  $2\frac{1}{2}$  inches



inches diameter, and  $2\frac{1}{2}$  inches long\*. This, being made red-hot, was stuck upon an iron handle fixed on a stand so as to present one of its circular faces to a lens placed at 2.8 inches distance; its focus being 1.4 inch, and diameter 1.1. Before the lens, at some distance, was placed a screen of wood, with a hole of an inch diameter in it, by way of limiting the object, that its image in the focus might not be larger than necessary. The screen also served to keep the heat from the thermometers. No. 2 was situated in the secondary focus of the lens; and No. 3 was placed within  $\frac{3}{8}$  of an inch of it, and at the same distance from the lens as No. 2. By this arrangement, both thermometers were equally within the reach of transmitted heat; or, if there was any difference, it could only be in favour of No. 3, as being behind a part of the lens which, on account of its thinness, would stop less heat than the middle. Now, as the experiment gives a result which differs from what would have arisen from the situation of the thermometers, on a supposition of transmitted heat, we can only ascribe it to a condensation of it by the refraction of the lens; and, in this case, the thermometer No. 3, by its situation, must have been partly within the reach of the heat-image formed in the focus. During the experiment, the thermometers were alternately screened two minutes from the effects of the lens, and exposed to it for the same length of time, and the result was as follows:

		No. 2. In the Focus.	No. 3. Near the Focus.	} Here, in the first and second minutes, No. 2 gained two degrees of heat more than No. 3. In the third and fourth, it lost one
Screened	0'	56	56	
Open	2	62	60	
Screened	4	59	58	
Open	6	61	59	
Screened	8	$58\frac{1}{4}$	$57\frac{3}{4}$	
Open	10	$59\frac{1}{2}$	$58\frac{1}{4}$	

more than No. 3. In the fifth and sixth, it gained one more. In the seventh and eighth, it lost  $1\frac{1}{2}$  more; and in the ninth and tenth, it gained  $\frac{3}{4}$  more than the other thermometer. This plainly indicates its being acted upon by refracted heat. Left there should remain a doubt upon the subject, I now re-

\* See Plate VIII. fig. 1.

moved the lens, and, putting a plain glass in the room of it, I repeated the experiment with all the rest of the apparatus in its former situation.

Screened	0'	57 $\frac{1}{4}$	56 $\frac{3}{4}$	} Here we find that both thermometers received heat and parted with it always in equal quantities, which confirms the experi- ment that has been given. And thus it is evident that there are rays issuing from red-hot iron, which are subject to the laws of refraction, nearly equal to those which affect light; and that these rays are invested with a power of causing heat in bodies.
Open	2	62 $\frac{1}{4}$	61 $\frac{3}{4}$	
Screened	4	60 $\frac{1}{2}$	60	
Open	6	61	60 $\frac{1}{2}$	
Screened	8	60	59 $\frac{1}{2}$	
Open	10	60 $\frac{4}{5}$	60 $\frac{1}{4}$	

*16th Experiment. Refraction of Fire-heat, by an Instrument resembling a Telescope.*

It occurred to me that I might use a concave mirror to condense the heat of the fire in the grate of my chimney, and, reflecting it sideways by a plain mirror, I might afterwards bring it to a secondary focus by a double convex lens; and that, by this construction, I should have an instrument much like a Newtonian telescope \*. The thermometer would figuratively become the observer of heat, by being applied to the place where, in the real telescope of the same construction, the eye is situated to receive light. Having put together the different parts, in such a way as I supposed would answer the end, I tried the effect by a candle, in order to ascertain the proper distance of the object-mirror from the bars of the chimney-grate. The front of the apparatus was guarded by an iron plate, with a thick lining of wood; and the two thermometers which I used, were parted from the mirrors and lens by a partition, which screened them from the heat that was to be admitted through a proper opening in the front plate, to come at the object-mirror. In the partition was likewise an opening, of a sufficient diameter to permit the rays to come from the eye-glass to their focus, on the ball of the thermometer No. 1; while No. 4 was placed

\* See Plate VIII. fig. 2.



by the side of it, at less than half an inch distance. In the experiment, the object-mirror was alternately covered by a piece of pasteboard, and opened again. The thermometers were read off every minute; but, to shorten my account, I only give the last minute of every change.

			No. 1.	No. 4.	} Here, in the
			In the Focus.	Near the Focus.	
The mirror covered	0		$77\frac{1}{2}$	$77\frac{1}{2}$	} first eight minutes, the thermometer exposed to the effects of the fire-
The mirror open	8		84	76	
Covered	-	16	$86\frac{1}{2}$	$79\frac{1}{2}$	
Open	-	21	$89\frac{3}{4}$	81	
Covered	-	27	$89\frac{3}{4}$	$82\frac{1}{4}$	
Open	-	37	$91\frac{1}{2}$	$83\frac{1}{2}$	
Covered	-	47	84	77	

instrument, gained 2 degrees of heat more than the other. In the next 8 minutes, the mirror being covered, it gained 1 degree less than the other. The mirror being now opened again, it gained, in five minutes,  $2\frac{3}{4}$  degrees more than the other. When covered six minutes, it gained  $1\frac{1}{4}$  degree less than No. 4. In the next ten minutes, when open, it gained  $\frac{1}{2}$  degree more; and, in the last ten minutes, when the fire began to fail, and the mirror was again covered, it lost one degree more than the other thermometer. All which can only be accounted for by the heat which came to the thermometer through the fire-instrument; and, as this experiment confirms what has been said before of the refraction of culinary heat, so it also adds to what has already been proved of its reflection. For, in this fire-instrument, the rays which occasion heat could undergo no less than two reflections and two refractions.

*17th Experiment. Refraction of the invisible Rays of Solar Heat.*

I covered one-half of Mr. Dollond's burning lens with pasteboard, and threw the prismatic spectrum upon that cover\*; then, keeping the last visible red colour one-tenth of an inch from the margin of the pasteboard, I let the invisible rays beyond the spectrum fall on the lens. In the

\* See Plate I. fig. 2.

focus of the red rays, or a very little beyond it, I had placed the ball of the thermometer No. 1; and, as near to it as convenient, the small one No. 2. Now, that the invisible solar rays which occasion heat were accurately refracted to a focus, may be seen by the following account of the thermometers:

	No. 1.	No. 2.	} Here, in one minute, these rays gave 45 degrees of heat to the thermometer No. 1, which received them in the focus, while the other, No. 2, suffered no change.
	In the Focus.	Near the Focus.	
o'	57	57	
I	102	57	

It is remarkable that, notwithstanding I kept the red colour of the spectrum  $\frac{1}{10}$  of an inch upon the pasteboard, a little of that colour might still be seen on the ball of the thermometer. This occasioned a surmise that, possibly, the invisible rays of the sun might become visible if they were properly condensed; I therefore put this to the trial, as follows:

*18th Experiment, Trial to render the invisible Rays of the Sun visible by Condensation.*

Leaving the arrangement of my apparatus as in the last experiment, I withdrew the lens, till the last visible red colour was two-tenths of an inch from the margin of the semicircular pasteboard cover; then, taking the thermometers, I had as follows:

	No. 2.	No. 3.	} Here there was no longer the least tinge of any colour, or vestige of light, to be seen on the ball of the thermometer; so that, in one minute, it received 21 degrees of heat, from rays that neither were visible before, nor could be rendered so by condensation.
o'	57	57	
I	78	57	

To account for the colour which may be seen in the focus, when the last visible red colour is less than two-tenths of an inch from the margin of the pasteboard which intercepts the prismatic spectrum, we may suppose that the imperfect refraction of a burning lens, which, from its great diameter, cannot bring rays to a geometrical focus, will bring some scattered ones to it, which ought not to come there. We may also admit that the termination of a prismatic spectrum cannot



cannot be accurately ascertained by looking at it in a room not sufficiently dark to make very faint tinges of colour visible. And to this must be added that the incipient red rays must actually be scattered over a considerable space, near the confines of the spectrum, on account of the breadth of the prism, the whole of which cannot bring its rays of any one colour properly together; nor can it separate the invisible rays entirely from the visible ones. For, as the red rays will be but faintly scattered in the beginning of the visible spectrum; so, on the other hand, will the invisible rays, separated by the parts of the prism that come next in succession, be mixed with the former red ones. Sir Isaac Newton has taken notice of some imperfect tinges or haziness on each side of the prismatic spectrum, and mentions that he did not take them into his measures\*.

*19th Experiment. Refraction of invisible Culinary Heat.*

There are some difficulties in this experiment; but they arise not so much from the nature of this kind of heat, as from our method of obtaining it in a detached state. A red-hot lump of iron, when cooled so far as to be no longer visible, has but a feeble stock of heat remaining, and loses it very fast. A contrivance to renew and keep this heat might certainly be made, and I should, indeed, have attempted to carry some method or other of this kind into execution, had not the following trials appeared to me sufficiently conclusive to render it unnecessary. Admitting, as has been proved in the 15th experiment, that the alternate rising and falling of a thermometer placed in the focus of a lens, when the ball of it is successively exposed to, or screened from, its effects, is owing to the refraction of the lens, and cannot be ascribed to a mere alternate transmission and stoppage of heat, I proceeded as follows†:—My lens, 1.4 focus, and 1.1 diameter, being placed 2.8 inches from the face of the heated cylinder of iron, the thermometer No. 2, in its focus, was alternately guarded by a small pasteboard screen put before it, and exposed to the effects of condensed heat by removing it.

\* Newton's Optics, p. 23, l. 11.

† See Plate VIII. fig. 1.

No. 2.			} Now, the beginning of this experiment being exactly like that of the 15th, with the thermometer No. 3 left out, the arguments that have before proved the refraction of heat in one state, will now hold good for the whole. For here we have a regular alter-	
Screened	0'	55		Very red-hot.
Open	2	$63\frac{1}{2}$		Red-hot.
Screened	4	58		Still red-hot.
Open	6	$60\frac{1}{2}$		Still red.
Screened	8	$57\frac{1}{2}$		A little red.
Open	10	$59\frac{1}{4}$		Doubtful.
Screened	12	$57\frac{1}{2}$		} Not visible in my room darkened.
Open	14	$58\frac{1}{2}$		
Screened	16	$57\frac{1}{2}$		
Open	18	$58\frac{1}{4}$		
Screened	20	$57\frac{1}{4}$		
Open	22	58		
Screened	24	$57\frac{1}{2}$		
Open	26	58		
Screened	28	$57\frac{1}{2}$		

nate rising and falling of the thermometer, from a bright red heat of the cylinder, down to its weakest state of black heat; where the effect of the rays, condensed by the lens, exceeded but half a degree the loss of those that were stopped by it.

*20th Experiment. Confirmation of the 19th.*

In order to have some additional proof, besides the uniform and uninterrupted operation of the lens in the foregoing experiment, I repeated the same with an assistant thermometer, No. 3, placed first of all at  $\frac{3}{4}$  of an inch from No. 2, and more towards the lens, but so as to be out of the converging pencil of its rays, and also to allow room for the little screen between the two thermometers, that No. 3 might not be covered by it.

		No. 2.	No. 3.	} Here No. 3, being out of the reach of refraction, gradually acquired its maximum of heat, in consequence of an uniform exposure to the influence of
		In the Focus,	Advanced sideways.	
		Always open.		
Screened	0'	$62\frac{1}{2}$	63	
Open	1	$63\frac{3}{4}$	64	
Screened	2	$62\frac{7}{8}$	64	
Open	3	64	$64\frac{1}{2}$	
Screened	4	$63\frac{3}{4}$	$64\frac{1}{2}$	
Open	5	$64\frac{7}{8}$	$64\frac{1}{2}$	
Screened	6	$64\frac{1}{2}$	$64\frac{1}{2}$	
Open	7	$64\frac{3}{4}$	64	
Screened	8	$64\frac{1}{2}$	64	

the hot cylinder; after which it began to decline. No. 2, on the



the contrary, came to its maximum by alternate great elevations and small depressions; and afterwards lost its heat by great depressions and small elevations. After the first eight minutes, I changed the place of the assistant thermometer, by putting it into a still more decisive situation; for it was now placed by the side of that in the focus, so as to participate of the alternate screening, and also to receive a small share of one side of the invisible heat-image, which, though unseen, we know must be formed in the focus of the lens. Here, if our reasoning be right, the assistant thermometer should be affected by alternate risings and fallings; but they should not be so considerable as those of the lens.

		No. 2. In the Focus.	No. 3. In the Edge of it.	} Here the changes of the thermo- meter No. 2 were $-\frac{3}{4} + \frac{1}{2}$ $-\frac{1}{4} + \frac{3}{4}$ $1 + 1$ ; and those of No. 3 were $-\frac{1}{4} +$ $\frac{1}{4} - \frac{3}{4} + \frac{1}{4} -$
Both open	8'	$64\frac{1}{2}$	64	
Both open	9	$63\frac{3}{4}$	$63\frac{3}{4}$	
Open	11	$64\frac{1}{2}$	64	
Screened	$12\frac{1}{2}$	63	$63\frac{1}{4}$	
Open	14	$63\frac{3}{4}$	$63\frac{1}{2}$	
Screened	16	$62\frac{3}{4}$	63	
Open	18	$63\frac{3}{4}$	$63\frac{3}{4}$	

$\frac{1}{2} + \frac{3}{4}$ . All which so clearly confirm the effect of the refraction of the lens, that it must now be evident that there are rays issuing from hot iron, which, though in a state of total invisibility, have a power of occasioning heat, and obey certain laws of refraction, very nearly the same with those that affect light.

As we have now traced the rays which occasion heat, both solar and terrestrial, through all the varieties that were mentioned in the beginning of this paper, and have shown that, in every state, they are subject to the laws of reflection and of refraction, it will be easy to perceive that I have made good a proof of the three first of my propositions. For the same experiments which have convinced us that, according to our second and third articles, heat is both reflexible and refrangible, establish also its radiant nature, and thus equally prove the first of them.

*End of the First Part.*

Slough, near Windsor,

April 26, 1800.

VII. Account.

VII. *Account of some interesting Experiments, performed at the London Philosophical Society, respecting the Effects of Heat, excited by a Stream of Oxygen Gas thrown upon ignited Charcoal, on a Number of Gems and other refractory Substances submitted to its Action; with a Description of the Apparatus employed.*

[Continued from Page 29.]

*Wedgewood's Pyrometer Pieces.*

XVII. **A**FTER the experiments already described, it was proposed by several of the members that some attempt should be made to determine the degree of heat which had been excited; and, with a view to this end, a fragment of one of Mr. Wedgewood's pyrometer pieces was subjected to the blast: in a few seconds, however, it became perfectly fused. It does not appear, therefore, that we are at present in possession of any better instrument for appreciating the intensity of heat thus produced than the gems themselves, which, as some of them are more refractory than others, and many of them more so than most other substances, may, from the changes which they undergo, serve to afford a rough method of estimating the comparative degree of temperature.

*Spinel.*

XVIII. A ruby-spinel, which weighed  $\frac{24}{84}$ ths of a carat, was, like the preceding stones, exposed in an excavation made in a piece of charcoal ignited by means of a common blow-pipe, to a stream of oxygen gas from the gasometer. At the end of 2' 3" it had lost neither weight nor colour, but had assumed the appearance of a rough garnet, having apparently undergone a very slight and superficial fusion, just sufficient to injure its polish.

*Jargoons.*

XIX. These stones are colourless, and harder than rock-crystal, but less so than the ruby. They are usually found in diamond-mines, and, when well cut and set, play nearly as well as rose diamonds. That they are however essentially



different from the diamond, needs no other proof than their incombustibility.

One which weighed  $\frac{14}{64}$ ths of a carat, exposed to the heat for 2', was found to have lost no weight; whereas a diamond of the same size would in that time have been entirely dissipated, and resolved into carbonic acid gas. The stone lost its transparency, and assumed the appearance of a piece of white enamel: it had evidently begun to fuse, but some traces of its form, and even of its facets, are still discernible.

XX. Another jargoon, weighing  $\frac{10}{64}$ ths of a carat, was exposed to the heat for the same length of time. It broke into three pieces while under the blast, but now presents the same appearances as the former. The surfaces of both are more opake and white than the interior.

#### *Vermilions.*

XXI. A vermilion which weighed  $\frac{8}{64}$ ths of a carat, treated for 2', lost no weight, but became fused into a polished opake globule, which is nearly black, with a tinge of brownish-green, like that of very dark bottle glass.

XXII. Fourteen vermilions, weighing  $\frac{48}{64}$ ths of a carat, exposed for 2' 30'', were completely fused into a polished opake globule, very like the former, but not quite so dark. It is also not so round or polished, having been pressed while soft. A small face being afterwards subjected to the lapidary's wheel, the stone appeared as hard as at first, but not susceptible of so high a polish.

#### *Garnet.*

XXIII. One which weighed  $1\frac{8}{64}$ ths of a carat, exposed to the blast for 1' 40'', broke in two pieces, both of which became fused. The largest presents an appearance very like that of the vermilion No. XXI.; the other is of a dark lead colour inclining to a bronzy hue and chatoyant, somewhat like peacock-lead ore. Both were pressed while soft; the latter so much so, that it at present retains nothing of a globular form.

#### *Emeralds.*

XXIV. One, of the weight of  $\frac{10}{64}$ ths of a carat, was fused in 2' into a globule of an opake white colour without any loss

loss of weight. It exactly resembles a pearl from a common oyster, or "dead pearl," as jewellers term it.

XXV. Another emerald, weighing  $\frac{2}{8}\frac{0}{4}$ ths of a carat, being treated for the same length of time, presents the same appearance as the former, except that about one-half of the globule still retains some traces of its original colour, and is now of a greenish gray.

*Chrysolite.*

XXVI. A chrysolite, weighing  $\frac{1}{8}\frac{6}{4}$ ths of a carat, being treated during 2' lost no weight, but became fused into a rough opake greenish black globule.

*Jacynth.*

XXVII. One jacynth, weighing  $\frac{4}{8}\frac{4}{4}$ ths of a carat, treated for 1' 14" lost no weight, but became fused into a globule much resembling one of dark coloured bottle-glass, but nearly opake.

*Opal.*

XXVIII. An opal, weighing  $\frac{1}{8}\frac{4}{4}$ ths of a carat, treated for 44" melted into a polished globule of a greenish white almost transparent, but full of small bubbles. It lost no weight.

*Crystals.*

XXIX. A cut white crystal, weighing  $\frac{2}{8}\frac{2}{4}$ ths of a carat, treated 1' 44" melted into a perfectly transparent globule like flint glass, but full of cracks and bubbles. Its weight is the same as before.

XXX. Two fragments of rock-crystal, weighing together  $1\frac{3}{4}$  carat, exposed to the heat during 5' 19" melted together, but flew to pieces on cooling, and the whole is now in several fragments; some of it even in powder. It resembles a very pure white salt.

*Platina.*

XXXI. Sixteen carats of platina in grains, in the crude state, were perfectly fused in a few seconds. A second and a third quantity were likewise fused; but the circumstance most remarkable was, that the globules thus formed were found to be pure and malleable. On examining them attentively it was found that the iron, which is always present  
in



in crude platina, and which, by adhering to it with great obstinacy, makes it difficult to obtain that metal in its pure state, was completely oxydated, and adhering so loosely to the surface of the globules that it separated itself in scales when they were struck with a hammer. The globules were then extended under the hammer without breaking.

From this experiment it appeared probable that means might be devised to fuse large quantities of crude platina, and at the same time to obtain the metal pure and malleable; an object so desirable, that the Society resolved at least to make the attempt.

XXXII. A table furnace was constructed in such a manner, that the crucible containing the platina (eight ounces) could be brought to a strong heat, by means of charcoal all round it and a pair of double bellows, before allowing the stream of oxygen gas to be introduced into it. The platina was put into the centre of the crucible, with charcoal below and all round, in such a manner that the crucible was filled with the charcoal and the metal. The cover was luted on, and a tube, made of burnt clay, which passed from a hole near the bottom of the crucible to the outside of the furnace, was firmly luted into the crucible, which was supported in the middle of the furnace. The clay tube was connected, by means of another tube, with the large gasometer, and that again with a series of casks filled with oxygen gas.

After exciting, by means of the double bellows, the fire round the outside of the crucible, till it was thought the crucible and its contents must have attained the utmost degree of heat that could in this way be obtained, the communication between the gasometer and the interior of the crucible was opened, and a stream of oxygen gas forced in through the clay tube among the contents of the crucible.

It will be observed from what has been stated, that the intention was not to excite the fire in the body of the furnace by means of a stream of oxygen. That appeared to be an unnecessary waste. All that was aimed at was, by exciting a sudden and rapid combustion of the charcoal lodged in the crucible along with the platina, to reduce the latter; and it was thought that the previous ignition of the crucible and its contents, by the help of the furnace, would facilitate this.

In a few seconds after opening the communication with the gasometer, such a vivid flame issued through the hole in the cover of the furnace, that it was thought unnecessary to carry the process further, as the gas appeared evidently to be acting on the fuel in the furnace, instead of having its action confined principally, as was intended, to the contents of the crucible.

After some time, the furnace was examined. More than a half of the clay tube which served to convey the gas through the furnace into the crucible was found gone, and the remaining portion of the tube polished highly on the surface, the clay having been running in a state of extreme fusion. The side of the crucible next the tube was polished in the same manner, and the hole in which the tube had been luted was found greatly enlarged. The charcoal in the crucible had not been half consumed, and the platina was found fused at the bottom. A few globules were found interspersed among the charcoal, having been arrested in their descent by stopping the process before they had time to reach the chief mass at the bottom; but it seemed plain that, had the blast of oxygen gas been continued for a minute or two longer, the crucible would have been completely run down.

The platina was then examined, but the result was not what the Society had promised themselves. Though it had been completely fused, the process was found to have been of too short a duration to oxydate all the iron contained in it. The large button broke under the hammer.

From this it appears that, though platina, in small quantities (Exper. XXXI.) may be reduced and brought to a state of purity by oxydating the iron by means of a stream of oxygen gas, it will require a considerable degree of address to be able to apply this on a scale of any considerable extent. If even rock-crystal, and those gems which, till these experiments, have been held infusible, are yet found fusible by the powerful heat excited by the agents employed in them, how are materials to be obtained sufficiently refractory to maintain the necessary arrangements in a furnace during such a process?



VIII. *An Examination of ST. PIERRE's Hypothesis respecting the Cause of the Tides, which, in opposition to the received Theory, attributes them to supposed periodical Effusions of the Polar Ices.* By SAMUEL WOODS, Esq. Read before the Askesian Society November 5, 1799.

[Concluded from Page 147.]

I NOW proceed to state the three remaining proofs adduced by St. Pierre in corroboration of the demonstration I have just noticed; but, as I conceive myself to have fully disproved the geometrical evidence, I shall not trouble you with an attempt to invalidate these subsidiary confirmations.

The second proof (says he) is atmospherical. It is well known that, in proportion as you ascend a mountain, the mercury in the barometer subsides: now the mercury sinks in the barometer in proportion as you advance northward. The weight of one line of mercury at Paris is equivalent to an elevation of 10 fathom and 5 feet, whereas in Sweden it is equivalent to 10 fathom 1 foot 6 inches only; and of course the ground of Sweden must be higher. From a series of observations made by captain Cook in the southern hemisphere in 1773 to 1775, we perceive the mercury scarcely ever rises higher than 29 inches beyond the 60th degree of south latitude, and mounted almost always to 30 inches and even higher in the vicinity of the torrid zone; which proves that the barometer falls as you recede from the line, and that both poles are elongated.

The third proof is nautical, arising from the annual descent of the ices toward the line, impelled by currents proceeding alternately from each pole during their respective summers, immense mountains of ice being frequently seen by navigators in low latitudes.

The fourth proof is astronomical. Childrey (an English author of note) supposes, as I do, that the earth at the poles is covered with ice to such a height as to render its figure sensibly oval. Kepler says that the eclipse of the moon on the 26th September 1624, like the one observed by Tycho Brahe in 1588, which was total, and very nearly central,

differed widely from the calculation : for, not only the duration of total darkness was extremely short, but the rest of the duration, previous and posterior to the total obscuration, was still shorter, as if the figure of the earth was elliptical, having the smallest diameter under the equator, and the greater from pole to pole.

Navigators in the north have always seen the elevation of the sun above the horizon greater the nearer they approach the poles. It is impossible to ascribe these optical effects to atmospherical refraction.

Barents, on the 24th of January, in Nova Zembla, saw the sun 15 days sooner than he expected, which would give a refraction of  $2\frac{1}{2}^{\circ}$ ; a thing impossible, and the circumstance can be ascribed to no other cause than his real elevation.

St. Pierre cuts the difficulty arising from the different vibrations of the pendulum, by observing that they are liable to a thousand errors.

The elongation of the poles being thus demonstrated, the current of the seas and tides follows as a natural and necessary consequence.

Let us now consider the extent of the polar ices, and the powers capable of effecting their solution.

The polar ices in the winter proper to each hemisphere are from six to seven thousand leagues in circumference; but in their summer, from two to three thousand.

The ices and snows form in our hemisphere, in January, a cupola, the arch of which extends more than two thousand leagues over the two continents, with a thickness of some lines in Spain, some inches in France, several feet in Germany, many fathoms in Russia, and beyond the  $60^{\circ}$  of north lat. of some hundred feet. Some ice islands were seen by Ellis from fifteen to eighteen hundred feet above the level of the sea, and they probably go on increasing to the pole to a height indeterminable. Hence the enormous aggregation of water, fixed by the cold of winter in our hemisphere, above the level of the ocean, is clearly perceptible; and to the periodical fusion of these vast masses the general movement of the seas and tides is justly ascribable. The ices at the south pole exceed in quantity those at the north; and two such  
bodies



bodies of ices, alternately accumulated and dissolved, at the two poles, must occasion a very perceptible augmentation of its waters at their return to it by the action of the sun, and a great diminution by their reduction to ice when the sun retires. It has been calculated that the earth and sea covered with ice, may be equalled to 1-10th of the whole ocean, and the height of the polar ices is at least 600 feet; a mass which in melting must add 1-10th, that is 60 feet, to the level of the ocean.

Nature has distributed sandy zones to assist, at the proper season, in accelerating the fusion of the polar ices. The winds in summer convey the igneous particles with which these zones are filled towards the poles, where they assist the sun's action on the ices.

The moon also dissolves ice by the humidity of the atmosphere. When the moon shines in winter nights in all her lustre it freezes very sharply, because the north wind checks the evaporating influence of the moon: but if the wind is stilled ever so little, you see the heavens covered with vapours which exhale from the earth, and you feel the atmosphere softened.

Nature having determined to indemnify the poles for the sun's absence, makes the moon pass toward the pole, which the sun abandons: she crystallises, and reduces into brilliant snows, the waters which cover it; she renders its atmosphere more refractive, that the sun's presence may be detained longer in it, and restored sooner to it: and hence also there is reason to conclude she has drawn out the poles of the earth in order to bestow on them a longer participation of the sun's influence. We may judge from analogy the general effect of the tides: A source discharging itself into a basin produces at the sides of that basin a backward motion or counter current, which carries straws and other floating substances up towards the source.

Charlevoix (*Hist. of New France*) tells us that, though the wind was contrary, he sailed at the rate of eight leagues a day up lake Michigan, against its general current, by the assistance of its lateral counter-currents.

M. de Crevœur assures us, that in sailing up the Ohio, along its banks he made 422 miles in 14 days, or ten leagues a-day,

a-day, by means of the counter-currents, which have always a velocity proportional to that of the principal current.

The particular effects observed in lakes and rivers communicating with icy mountains, illustrate the nature of the polar effusions. A kind of flux and reflux in the lake of Geneva, during summer and towards the evening, is observable, occasioned by the melting of the snows, which fall into it after noon in greater quantities than at other seasons of the day. The intermittence of certain fountains is ascribable to the same cause. The frequent and rapid fluxes (ten or twelve times a day) of the Euripus, the strait separating Bœotia from Eubœa, arise from the same source.

The currents of the ocean are reducible to two general ones: one, during our summer, from the north pole, in a south direction; the other, during our winter, proceeding northward from the south pole.

Dampier lays it down as a principle, founded on many experiments, that currents are scarcely ever felt but out at sea, and tides upon the coasts.

The polar effusions, which are the tides of the north and east to those who dwell in the vicinity of the pole, or in bays communicating with it, take their general course to the middle of the channel of the Atlantic ocean, attracted toward the line by the diminution of the waters, which the sun is incessantly evaporating. They produce by their general current two contrary currents or collateral whirlpools similar to those produced by rivers on their banks, and the tides may be considered as vortices of the general current of the Atlantic ocean.

The general current, which flows from our pole in summer with so much rapidity, and which is so violent towards its source, crosses the equinoctial line, its flux not being stemmed by the effusions of the south pole, at that season consolidated into ice; it extends beyond the Cape of Good Hope, and being directed east, by the position of Africa and Asia, forces the Indian ocean into the same direction, and may be considered as the prime mover of the western monsoon, which takes place in the Indian seas in April, and ends in September.

The general current, issuing during our winter from the



south pole, restores the Indian ocean to its natural motion west; crosses, in its turn, the equinoctial line, penetrates into our Atlantic ocean, directs its motion north by the position of America, and produces various changes in our tides. All the bays, creeks, and mediterraneans of southern Asia, such as the gulphs of Siam and Bengal, the Persian gulph, the Red sea, &c. are directed relatively to these currents north and south so as not to be stemmed by them; as all the bays and mediterraneans of Europe, as the Baltic, the English channel, the bay of Biscay, the Mediterranean sea, Baffin's bay, Hudson's bay, the gulph of Mexico, and many others, are directed relatively to these currents east and west; or, to speak with more precision, the axes of all the openings of the land in the old and new world are perpendicular to the axes of these general currents, so that their mouth only is crossed by them, and their depth is not exposed to the impulsions of the general movements of the ocean.

That these currents are not the offspring of my own imagination, but actually such as I have described them, will appear from various testimonies. Froger says that in Brazil the currents follow the sun, running southward when he is south, and northward when he is north. In the summer of the southern hemisphere, the tides set in northward (*Schouten, Jan. 1661*), but in winter run southward and come from the north (*Frazer, May 1712*). C. Columbus set sail from the Canaries the beginning of September, and steered to the west; he found, during the first days of his voyage, that the currents carried him to the north-east; when he had advanced 200 or 300 leagues from land, he perceived their direction was southward: finally, as he approached the Lucayo islands, he again found the current setting in north.

The nautical observations of Cook demonstrate that the currents of the Atlantic ocean are alternate and half-yearly like those of the Indian ocean. The beans called Oxeyes, which grow only in the West Indies, are every year thrown up on the coast of Ireland, 1200 leagues distant. Seeds and turtles are brought to the Hebrides from the West Indies and America; and the mast of the Tilbury man of war, burnt at Jamaica, was found on these coasts: the current  
which

which wafts these along proceeds in a north direction, and proves that the Atlantic current comes from the south, and sets in north during our winter. The currents of the north annually convey, in summer, toward the south, long banks of floating ices of very considerable depth and elevation, which run aground as far south as the banks of Newfoundland.

Rennefort (June 20, 1666), near the Azores (in lat.  $40^{\circ}$  to  $45^{\circ}$ ), saw the broken masts, sailyards, &c. wrecked in the engagement which lasted four days between the English and Dutch, from June 11 to 15: this naval combat took place 12 miles to the north-west of Ostend, about  $51^{\circ}$  north. The currents from the north had therefore wafted them in nine days  $11^{\circ}$  south, besides a considerable progress westward.

The general current issuing from the south pole divides into two branches; one, setting in towards the Atlantic ocean, penetrates even to its northern extremity. This part, straitened by the prominent parts of Africa and America, forms on the coast two counter currents, which proceed in opposite directions. One of these currents runs east, along the coast of Guinea, to the 4th degree of south latitude; the other takes its departure from Cape St. Augustin, proceeding south-west, along Brasil, to Maire's Straits. In the middle of the Atlantic ocean, beyond the strait formed by the two continents, this general branch pushes on north, and advances to the north extremities of Europe and America, bringing us twice every day along our coasts the tides of the south, which are the half daily effusions of the two sides of the south pole. The other branch takes a direction south of Cape Horn, rushes into the South sea, produces the monsoon in the Indian ocean, and, having made the tour of the globe, unites itself by the Cape of Good Hope to the general current which enters the Atlantic ocean.

In our summer, commencing toward the end of March, when the sun retires from the southern hemisphere, and proceeds to warm the north, the effusions of the south pole are stayed, those of our pole begin to flow, and the currents of the ocean change throughout every latitude. The general current of our seas divides also into two branches; the first deriving its source from Waigats, Hudson's Bay, &c. flows  
with



with the rapidity of a sluice, descends through the Atlantic ocean, crosses the line, and, finding itself confined at the same Strait of Guinea and Brasil, forms two lateral counter currents setting in north: these counter currents produce, on the coasts of Europe, the tides which appear to come from the south. The general current advances south, arrives about the month of April at the Cape of Good Hope, and renders the passage round this cape so difficult to vessels returning from India at this season; about the middle of May it reaches the coasts of India, produces the west monsoon, and, having encompassed the globe, proceeds to Cape Horn, re-ascends the coasts of Brasil, and creates a current terminating at Cape St. Augustin.

The other general branch, which receives much less of the icy effusions, issues between the continents of Asia and America, and descends to the South sea, where it is re-united to the first branch. The ocean accordingly flows twice a year round the globe in opposite spiral directions, taking its departure alternately from each pole, and describes on the earth the same course which the sun does in the heavens.

The course of our tides towards the north in winter is not an effect of the lateral counter currents of the Atlantic ocean, but of the general current of the south pole, which runs north. In this direction almost throughout it passes from a wider space into a narrower, and carries before it at once the whole mass of the waters of the Atlantic ocean, without permitting a single column to escape either to the right or left. However, if it meet a cape or strait to oppose its course, it would form there a lateral current, as at Cape St. Augustin, and in Africa about  $10^{\circ}$  N. lat.; for in the summer of the south pole the currents and tides return south on the American, and east on the African side, the whole length of the Gulph of Guinea, in contradiction to all the laws of the lunar system.

From these polar effusions the principal phænomena of the tides may be explained. It will be evident, for example, why those of the evening should be stronger in summer than those of the morning; because the sun acts more powerfully by day than by night on the ices of the pole on the same meri-

dian as ourselves: and also why our morning tides in winter rise higher than those of the evening, and why the order of our tides changes every six months; because, the sun being alternately towards both poles, the effect of the tides must be opposite, like the causes which produce them. At the solstices the tides are lower than at any other season of the year, and those likewise are the seasons when there is most ice on the two poles, and consequently least water in the sea: the reason is obvious, the winter solstice is with us the season of the greatest cold; of course there is the greatest possible accumulation of ice on our pole and hemisphere. At the south pole it is indeed the summer solstice; but little ice is then melted, because the action of the greatest heat is not felt there as with us, till the earth has an acquired heat super-added to the sun's action, which takes place the six weeks following the summer solstice.

At the equinoxes, on the contrary, we have the highest tides; and these are precisely the seasons when there is least ice at the two poles, and of course the greatest quantity of water in the ocean. At our autumnal equinox in September, the greatest part of the ices of the north pole is melted, and those of the south pole begin to dissolve. The tides in March rise higher than those in September, because it is the end of summer to the south pole, which contains much more ice than ours, and consequently sends a greater mass of water to the ocean.

I shall say nothing (he proceeds) of the intermittence of the polar effusions, which produce on our coast two fluxes and two refluxes nearly in the same time that the sun, making the circuit of the globe, alternately heats two continents and two oceans, that is, in the space of 24 hours, during which his influence twice acts and is twice suspended; nor shall I speak of the retardation, which is nearly  $\frac{3}{4}$  of an hour every day, and which seems regulated by the different diameters of the polar cupola of ice, whose extremities, melted by the sun, diminish and retire from us every day, and whose effusions must consequently require more time to reach the line, and to return from the line to us. Nor shall I dwell on the other relations these polar periods have to the phases of the moon, especially



especially when she is at full; for her rays possess an evaporating heat, as the late experiments at Rome and Paris fully demonstrate\*; much less shall I involve myself in a discussion of the tides of the south pole, which in summer in the open sea come in vast surges from the south and south-west. There are two tides every day; because the sun warms by turns, every 24 hours, the east and west side of the pole in fusion. Precisely the same effect takes place in lakes situated in the vicinity of icy mountains, which have a flux and reflux in the day-time only. But it cannot be doubted that, if the sun warmed, during the night, the other side of these mountains, they would produce another flux and reflux; and consequently two tides in 24 hours, like the ocean.

We are not to imagine that every tide is a polar effusion of the particular day on which it happens, but an effect of the series of polar effusions; so that the tide which takes place on our coasts to-day, is perhaps part of that which took place six weeks ago. But here, too, must we admire the harmony of nature: the evening and morning tides take place on our coasts as if they issued that very day from the higher and lower part of our hemisphere; and the tides of summer are precisely opposite to the tides of winter, as are the tides from whence they flow; our evening tides in summer, and our morning tides in winter, being greatest.

If the tides are stronger after the full moon, it is because that luminary increases by her heat the polar effusions, and consequently the quantity of water in the ocean.

Let us now explain why the tides of the South sea do not resemble those of the Atlantic ocean. The irregular effusions of the poles, not being narrowed in the southern hemisphere, as in ours, produce on the shores of the Indian ocean and South sea expansions vague and intermitting. The south pole has not, like the north pole, a double continent, which separates into two the divergent effusions daily produced by the sun: it has no channel in passing through which its effluxes should be retarded: its effusions accordingly flow directly into the vast southern ocean, forming on the half of that pole a series of divergent emanations which perform the

\* I do not know where any account of these strange experiments can be found.—W.

tour, of it in 24 hours, like the rays of the sun. When a bundle of these effusions falls upon an island, it produces there a tide of twelve hours, *i. e.* of the same duration with that which the sun employs in heating the icy cupola through which the meridian of that island passes; such are the tides of the islands of Otaheite, Massafuero, New Holland, New Britain, &c.: each of these tides lasts as long as the course of the sun above the horizon, and is regular like his course.

In the northern part of the South sea the two continents approach: they pour therefore by turns, in summer, into the channel which separates them, the two semi-diurnal effusions of their pole, and there they collect by turns, in winter, those of the south pole, which produces two tides a day as in the Atlantic ocean. But as this channel about the  $55^{\circ}$  of N. lat. ceases to exist by the sudden divergence of the continents of Asia and America, those places only situated in the point of divergence of the northern parts of these two continents experience two tides a day. Such are the Sandwich Islands. Where such places are more exposed to the current of the one continent than the other, its two semi-diurnal tides are unequal, as at the entrance of Nootka Sound: but when it is completely out of the influence of the one, and entirely under that of the other, it receives only one tide of twelve hours every day, as at Kamtschatka. Thus, two harbours may be situated in the same sea under the same parallel, and one of them have two tides, and the duration of these tides, whether double or single, double equal or double unequal, regular or retarded, is always 12 hours every 24 hours, *i. e.* precisely the time the sun employs in heating that half of the polar cupola from whence they flow; which cannot possibly be referred to the unequal course of the sun between the tropics, and much less to that of the moon, which is frequently but a few hours above the horizon of such harbours. All islands are in the midst of currents: on looking therefore at the south pole with a bird's-eye view, we should see a succession of archipelagos dispersed in a spiral line all the way to the northern hemisphere, which indicates the current of the sea, just as the projection of the two continents on the side of the north pole indicates the current of the Atlantic. Thus,  
the



the course of the seas from one pole to the other is in a spiral line round the globe, like the course of the sun from one tropic to the other: admitting therefore the alternate fusion of the polar ices, all the phænomena of the tides and currents of the ocean may be explained with the greatest facility.

I have then established by facts simple, clear, and numerous, the disagreement of the tides in most seas with the moon's action on the equator, and their perfect coincidence with the sun's action on the polar ices.

I have no doubt various objections may be urged against this hasty explanation of the course of the tides, &c. But these physical causes present themselves with a higher degree of probability, simplicity, and conformity to the general progress of nature, than the astronomical causes by which it is attempted to explain them.—Thus far St. Pierre.

I hesitate whether I ought not to apologise for occupying so much of your time and attention in the detail of a theory which may be deemed unworthy of serious notice; yet I flatter myself it will afford an opening to curious and interesting discussion. St. Pierre complains that the prejudices of mankind are so strong in favour of received opinions, that he cannot obtain a hearing.

To the best of my judgment I have offered a fair and candid exposition of a hypothesis which he has dressed up with some eloquence and much declamation, and ushered into the world with a solemn and imposing air of confidence and assurance, tolerably well-calculated to confound the ignorance and candour of his readers. I am not conscious of having omitted any material fact or argument which tends to the support and elucidation of his theory; I have neglected much absurd reasoning, yet not without retaining some curious specimens. I did once intend to have entered into a general examination of his principles and reasoning; to have shown the fallacy of the former, the inconclusiveness and inconsistency of the latter; but I shall now be satisfied with offering a few facts and observations extracted from the 2d and third voyage of Captain Cook, which appear to me decisive of the question.

Captain

Captain Cook, who spent three summers as near as the ice would permit his approach towards the south pole, found on December 14, 1772, and from that date to the beginning of January 1773, in latitude from  $55^{\circ}$  to  $64^{\circ}$  south, a vast compact body of ice which prevented his further progress. The thermometer varied from  $30^{\circ}$  to  $35^{\circ}$ . Being immersed 100 fathom deep for about 20 minutes, it came up  $34^{\circ}$ ; and on the 13th of January 1774, on a repetition of this experiment, the open air being  $36^{\circ}$ , the surface of the sea  $33\frac{1}{2}^{\circ}$ , the thermometer came up  $32^{\circ}$ . They found water generally freeze at  $33^{\circ}$ . "We certainly had no thaw, (says he,) the mercury keeping usually below the freezing point. Being near an island of ice (December 24, 1772) 50 feet high and 400 fathom in circuit, I sent the master in the jolly boat to see if any water ran from it. He soon returned with an account there was not one drop, or any other appearances of thaw." And in the summer of 1774—75 his experience was nearly similar. On the 13th of February 1775, the thermometer stood at  $29^{\circ}$ . In his 3d voyage to the north-west coast of America, on the 17th of August 1778, in lat.  $70^{\circ} 44'$ , they were stopped by a field of ice 10 or 12 feet high, as compact as a wall; "further north it appeared much higher; here and there we saw upon it pools of water; we tried, but found no current. July 7, 1779, lat.  $69^{\circ}$ ; stopped by a large field of ice, presenting a great extent of solid and compact surface not in the smallest degree thawed: the thermometer stood at  $31^{\circ}$ ."

"As far as our experience went, the sea is clearer of ice in August than in July, and perhaps it may be still freer in a part of September. We tried the currents, and found them never to exceed a mile an hour; we found the month of July infinitely colder than August; the thermometer in July was once  $28^{\circ}$ , and very commonly  $30^{\circ}$ ; whereas it was seldom as low as the freezing point in August."

"I am of opinion (says Captain Cook) that the sun contributes very little towards reducing these vast masses of ice; for, although that luminary is a considerable time above the horizon, it seldom shines out more than a few hours at a time, and often is not seen for several days in succession. It is the wind, or rather the waves raised by the wind, that reduces the



the bulk of these enormous masses, by grinding one piece against another, and by undermining and washing away those parts that lie exposed to the surge; and more ice may be destroyed in one stormy season than is formed in several winters, and its accumulation thus prevented."

This evidence clearly proves that the sun's influence at the poles, so far from being equal to produce a constant and uniform effect, creating an impulse extending its effect to the remotest parts of our globe, and a daily elevation of several feet to the waters of the ocean, is not sufficient in the hottest period of summer to diffuse a sensible thaw; and thus we are convinced that a few plain and simple facts are of much greater avail than a multitude of fanciful conjectures.

## NEW PUBLICATION.

*A Manual of a Course of Chemistry; or, a Series of Experiments and Illustrations necessary to form a complete Course of that Science.* By J. B. BOUILLON LAGRANGE, Professor in the Central Schools of Paris, &c. Translated from the French, with 17 Plates. 2 Vols. 18 Shillings. Cuthel, and Vernor and Hood, 1800.

**W**E have perused this work with much pleasure. It is comprehensive, though concise; and the manner in which the author treats his subject is well calculated to give to those who wish to study chemistry, not only a knowledge of the theory, but also, which ought to be the chief aim of every work of this kind, of the processes and manipulations as applicable to the arts and the common purposes of life.

The apparatus employed in modern chemistry is described with considerable accuracy and clearness, and illustrated with appropriate engravings; but in the original, the connection and relation of all the parts, though very correctly given *in outline*, are not sufficiently apparent and obvious, especially to tyros. The English publishers have done justice to the work, and even a service to the science of chemistry, by having their plates properly shaded and highly finished. It is enough  
of

of them to say, that they are from the graver of LOWRY, and equal in merit and execution to those given in the Philosophical Magazine.

The translator, who has not given his name, has performed his task in a manner creditable to himself. He has shown himself something more than a mere literary drudge: he has corrected several oversights of the author, or rather, perhaps, inaccuracies of the French printer, which, though often only a single letter in the name of a compound body, were of importance. The terminations in *ates*, and *ites*, and *ats*, so useful in the modern nomenclature, have this inconvenience—they are so nearly similar, that printers, not chemists, will often substitute the one for the other. We speak this from experience, and we may be allowed to take this opportunity of recommending to chemical authors always to revise after the printer before their sheets are put to press.

A short appendix has been added by the translator, containing some useful notes and notices of new facts discovered since the original work was published; and also several tables of French weights, &c.

We shall here give a few extracts, which, at the same time that they serve as a specimen of the work and of the translation, will furnish information or amusement to some of our readers.

#### *Fluoric Acid.*

For our knowledge of the fluoric acid we are indebted to Scheele.

This appellation was given to it because it is extracted from a kind of earthy neutral salt, known under the name of *sparry fluor*, *phosphoric mineral*, *fluat of lime*.

As the fluoric acid dissolves glass, it is necessary, when you wish to have it pure, to employ for that operation vessels of a metal on which neither it nor the sulphuric acid exercises any action: lead, among the known metals, is that which is best fitted for this purpose.

There are two methods of preparing this acid: 1st, With a metallic apparatus: 2d, With a glass apparatus.

1st, To obtain the fluoric acid alone, and free from every combination, put one part of the fluat of lime, reduced to powder,



powder, into a leaden retort; pour over it three parts of sulphuric acid, and adapt to the retort a leaden receiver half full of water. The operation is performed with a *balneum mariæ*; and for this purpose you put the retort into a copper or iron vessel containing water, or into a salt-bath; you then expose the apparatus to a gentle heat, and the fluoric acid, in proportion as it is disengaged, will be absorbed by the water in the receiver.

For this purpose, instead of a receiver, you adapt to the orifice of the retort a bent tube of lead, the extremity of which is introduced under a mercurial pneumatic apparatus.

2d, When this acid is made by means of a glass apparatus, you employ a retort, having adapted to it a tube inserted in a bottle containing distilled water.

As this acid has the property of dissolving glass, it seizes on the flint, which appears under the form of white flakes.

Care must be taken to employ large tubes, especially when you operate with glass, as for want of a sufficient passage the gaseous acid is compressed in the retort, and its action on the glass is augmented, so that the retort will be soon corroded through.

The flint deposits itself in the water, because the latter has more affinity for the acid than the acid has for the flint.

If you preserve some of this gas under a glass bell, it will dissolve the flint of the glass.

If you plunge into it an extinguished taper, an incrustation will be formed on the wick, because the water which issues from it dissolves the surrounding acid charged with flint, and the siliceous earth is precipitated upon it from this solution.

This gas is heavier than atmospheric air; it extinguishes a lighted taper, kills animals, reddens blue vegetable colours, and has a penetrating smell, which approaches near to that of muriatic acid gas.

It corrodes the skin: it undergoes no alteration from light.

In contact with the air it emits white fumes. If you expose to the vapour of this gas in an earthen vessel animals, bits of sponge a little moistened, charcoal, &c. the moisture

they contain will dissolve the acid, and the filex will be precipitated upon them.

If this experiment be made in a metal vessel, the same incrustation will not take place.

It thence results that the earthy substance which is precipitated by the contact of the fluoric acid gas and the water, is nothing else than a portion of the glass, which is attacked and actually dissolved by the aëriform acid.

It often happens also, that when this gas is made to pass into water, the filex is precipitated in a quartzy pellicle. Each bubble of the acid which touches the water is immediately enveloped in filex, and leaves on its way as it ascends to the surface of the water, traces in the form of tubes, which Priestley calls *organ-pipes*, and which decrease to a point upwards, because the bubble decreases in proportion as the water dissolves it, and the filex is thus carried off.

The filex deposited in the vessels is soon after re-dissolved by the excess of acid, in proportion as the water is saturated; for the water, being at first little saturated with the acid, has not sufficient strength to hold the filex in solution.

Bergman obtained fluat of filex in a crystallized form.

When the fluoric acid is made in earthen vessels, you will have filex deposited, and then re-dissolved by the re-action of the acid: this is a real fluat of filex instead of pure fluoric acid.

Alkalies may be employed for detecting the presence of the filex. The taste of this acid, dissolved in water, is like that of the sulphuric acid diluted with water, or of vinegar.

If a solution of fluoric acid in water be exposed to heat, part of the acid is volatilized; but the last molecule adhere so strongly, that the water and the rest of the acid are volatilized, if the heat be increased.

This acid is preserved in bottles covered on the inside with wax dissolved in oil, or in vessels of lead or of platina.

Guyton employed this property to engrave labels on bottles, and in particular on those destined to hold acids, the labels of which when made of paper are always burnt.

The elements of this acid are still entirely unknown,

[To be continued.]

INTEL-



## INTELLIGENCE,

AND

## MISCELLANEOUS ARTICLES.

*LEARNED SOCIETIES.*

## ROYAL SOCIETY OF LONDON.

**T**HE fittings of the 17th November, the 11th and 18th December, were occupied in reading an interesting paper on the mechanism of the eye, by Dr. Young.

St. Andrew's day falling on a Sunday, the anniversary meeting was held on the 1st of December. The auditor's report on the state of the finances of the Society was read. The names of the new members, those deceased, and those withdrawn, were declared; and the names of the officers just elected were announced. The Right Hon. President then addressed the Society, congratulating them on the progress of science in the last year, and concluded by informing them that Sir Godfrey Copley's medal was adjudged to Mr. H. Howard for his paper on fulminating mercury. Sir Joseph Banks then informed the Society that Mr. Howard had also discovered a new fulminating silver, and is now engaged on a subject that promises to prove highly interesting to meteorologists and mineralogists, namely, the analysis of stones that have fallen from the clouds.

SOCIETY FOR THE ENCOURAGEMENT OF ARTS,  
MANUFACTURES, AND COMMERCE.

This Society, which has rendered much service to the arts by its numerous premiums, that its efforts may, if possible, be rendered still more generally useful, has just circulated the following notice:

Adelphi, Dec. 8, 1800.

*Premiums by the Society for the Encouragement of Arts,  
Manufactures, and Commerce.*

The time of proposing and publishing the premiums to be given by this Society for the ensuing year now approaching, the patrons of the arts, and all ingenious men, are hereby invited to suggest new objects, to which the Society may extend its liberality.

Communications are requested to be made in writing, and addressed to the Society's secretary, at their house in the Adelphi, on or before the 1st of February 1801.

N. B. The 18th volume of the Transactions of this Society is now in the press, and will speedily be published.

By order, CHARLES TAYLOR, Sec.

## BOARD OF AGRICULTURE.

This Board has just published the following list of premiums:

This Board having been required, by a committee of the House of Lords, 'to examine into, and report to their lordships, the best means of converting certain portions of grass land into tillage, without exhausting the soil, and of returning the same to grass, after a certain period, in an improved state, or at least without injury;' and being desirous that their information, on a subject of so great importance, should be complete, adapted to every sort of soil, and founded on the most ample experience, have come to the resolution of offering the following premiums for that purpose, *viz.*

To the person who shall produce, on or before the first day of February 1801, the best and most satisfactory essay on the subject before mentioned, distinguishing, respectively, what part of the plan recommended, or of the details given, is the result of actual experiment, accurate observation, or well authenticated information, 200*l.* For the second best, 100*l.* For the third best, 60*l.* For the fourth best, 40*l.*

And to such persons who may communicate information, which, though useful, may be considered of less importance, smaller rewards, proportioned to the opinion of the Board.



It is required that each essay shall fully detail the course of crops, regard being had to the varieties of soil, and the time proposed for continuing the land under tillage.

Also, to explain the cases in which it may be eligible to drain land previous to tillage.

In what cases paring and burning are advantageous, with directions thereon, regard being had to the subsequent cropping.

The depth to which grass lands should, at first breaking up, be ploughed.

Whether the crops intended for cattle and sheep are to be fed on the land, and by which kind of stock, or carted off.

To state the crop with which the grass seeds in each case ought to be sown, when the land shall be again laid down:—The sorts and quantities of grass seeds for each kind of soil, and whether to be provided by landlord or tenant:—Whether it be best to mow or feed the grass in the first year after laying down; to detail the management in each case:—The manuring which may be thought necessary:—The principle on which an increase of rent ought to be estimated, where permission may be given to break up old pasture now under lease.

The Board requires that these objects should be particularly attended to, with relation to the leading qualities of land, *viz.*—Clay, in all its distinctions; and soils too strong or wet for turnips:—Loam, in all its distinctions, fit for turnips:—Sand, including warrens and heaths, as well as rich sands:—Chalk land and downs:—Peat, including moory, sedgy, rough bottoms, and fens.

It is hoped that no person will be deterred from communicating his knowledge to the Board on account of his experience being confined to one of these soils only.

The Board reserves the power of withholding any premium, in the case of no essay being deemed sufficiently important to merit it.

The essays which shall obtain any premium, or other reward, to remain the property of the Board.

Each essay to be sent (sealed) without any name, but with a mark or motto; and accompanied by a sealed letter with the same mark or motto, containing the name and address of the

the

the author; and this letter will not be opened, unless one of the prizes, or some other reward, shall be adjudged to him.

All communications to be addressed to Lord Carrington, president, Sackville-street.

#### FRENCH NATIONAL INSTITUTE.

The following account of the labours of the Physical and Mathematical Class during the last three months of the year 8 was read by C. Delambre.

C. Meissier has given a curious comparison of the summer of the year 1800 and that of the year 1792. It results from his observations, that if the duration of the heat was nearly the same at each of these periods, it is certain that in the year 1792 the thermometer kept, pretty constantly, two degrees and a half higher than in the year 1800; but what renders the last summer more remarkable is, the extraordinary lowness of the water of the Seine.

In the year 1792, the water, for two months and a half, kept, pretty constantly, at about an inch above zero of the scale on the bridge de la Tournelle. During four days only, it sunk so low as zero. On the 11th of August last it was still lower by 4.9 inches. On the 20th of August it was even 7.65 inches below zero. It appears, then, that in the year 1800, the depression of the water of the Seine was something more than six inches greater than it was in 1792.

The year 1719 was remarkable also for a long drought and depression of the river. During the whole of that year there fell no more than nine inches four lines of water at the observatory of Paris, which is not the half of the usual quantity. The depression of the water appeared then so extraordinary, that, to preserve the remembrance of it, the scale of the bridge La Tournelle was constructed. This precaution, however, did not prevent us from having 325 millimetres of uncertainty in regard to the level of the Seine at that period. According to Parcieux, the depression of the water in 1719 was indicated by the zero of the scale. According to Buache, it corresponded to the first foot of that scale. Meissier is inclined to adopt the latter indication. But however this may be, it is certain that in the year 1800 the river fell lower

than



than at any period ever remembered. According as we follow the one or the other of the two authors above mentioned, the difference will be 16 or 48.

C. Meiffier read also a note on the eclipse of the moon which took place on the first of October last. The clouds prevented him from observing the commencement of it. Towards the middle the moon appeared at certain intervals; and he took advantage of these moments to measure twelve times the part eclipsed. At 11 h. 21'' the eclipse seemed to be at an end: it was more certainly so at 11 h. 0' 40''. According to the observations of C. Delambre, the end took place about 11 h. 0' 30''; which agrees pretty well with what might have been expected.

C. Prony read a memoir containing a description and the analytical theory of a new instrument, proper for measuring the length of a pendulum that swings seconds. It is well known, that the usual method requires the nicest attention, not only in regard to the form of the body made to oscillate, but also that the oscillations may not be prolonged beyond two hours at most, and that the results deduced from them may be always subordinate to the regularity of the time-piece which serves for comparison; and in the last place, the necessity of fitting up, and taking to pieces the apparatus, may leave some doubts respecting the identity of the experiments made at different times, and in different places. All these inconveniences C. Prony proposes to remedy. His new instrument supersedes the necessity of paying attention to the form of the oscillating body; the mass of it is too considerable for the oscillations to continue during the whole time that elapses between two consecutive passages of a star over the same vertical, so that the time-piece of comparison is of no further use than to count with more facility the oscillations of the pendulum used for the experiment.

The theory of C. Prony appears to be clear and satisfactory, and he soon intends to subject his new pendulum to a trial. The ingenious method by which Borda found means to correct or obviate the inconveniences of the common method are not yet forgotten. His determination of the length of the pendulum received the approbation of all learned

learned men, assembled at Paris, for fixing the new measures. If it be difficult to attain to greater precision, it will at least be interesting to see how far the results obtained are confirmed by a method absolutely different.

C. Flaugerges read several memoirs, containing observations on various subjects; as, on the phosphorescence of earth worms; on the effects of thunder; on halos, or luminous rings around the sun; experiments tending to prove that the shadows of opaque bodies, exposed in the open fields to the light of the sun, when the sky is serene, and projected on a white surface, are always blue, or, more correctly, of the colour of the heavens: and, in the last place, experiments from which it results, that the waves, produced at the surface of the water by percussive, do not produce a movement of translation in the parts of the fluid, as has been believed, according to Newton, but only a continued depression of intumescence, which is afterwards propagated circularly.

C. Briffon has published *Physical Principles of Chemistry*, destined as a supplement to his *Principles of Natural Philosophy*. The author composed this work chiefly for the use of the students in the central schools. It contains, in a clear and methodical order, an account of all the substances which fall within the province of chemistry, an analysis of them, their specific gravities and other most remarkable properties, with a description of the principal kinds of apparatus employed for chemical experiments\*.

C. Lacroix has published his *Treatise of Differences and Series*, forming a continuation of his *Treatise on the Differential and Integral Calculus*. This important work comprehends in one system the methods scattered throughout different academical collections, and in the works of the greatest geometricians of modern times. The same author has published a second edition of his *Treatise of Trigonometry*, and the application of Algebra to Geometry.

C. Arbogust has just published a large work, entitled *The Calculus of Derivations*. The author gives the name of derivations to quantities deduced from each other by an uniform

\* We are happy to learn that a translation of this very useful work is now in the press, and will soon be published.



process, which has a great resemblance to that used for finding the differentials of all the orders.

This process he applies to different polynomous quantities; he teaches the method of finding the derivations in their greatest simplicity, and of forming them almost without any other trouble than that of writing them. Each term of the resolution may be obtained directly, and without being obliged to pass through the intermediate ones.

This new calculus may be applied to simple and double recurring series, and to the general recurrence of series. It will serve often also to give more generality to known theorems, more vigour and brevity to demonstrations; in a word, it is of the greatest utility in the differential and integral calculus, which indeed forms only a peculiar case of the calculus of derivations.

The foreign members of the commission of weights and measures continue their information respecting the interest with which their different governments receive the models of the metre and chiliogramme. M. Bugge, director of the observatory and of the board of longitude at Copenhagen, informs us that these two models have been committed to his care. C. Tralles, the deputy from Helvetia, and the minister of arts and sciences for that republic, gives reason to hope that the metric system will be adopted in that country. All these letters have been printed. We must not here omit a circumstance which adds to our hopes in regard to Helvetia; which is, that, by a fortunate chance, there exists a very simple and remarkable relation between the metre and the Zurich foot, one of the most usual measures in Switzerland. This foot is exactly equal to three decimetres, or at least it does not exceed it but by two centi-millimetres, that is to say, by a little less than the hundredth part of a line—a quantity that may be neglected in commerce, and which is often inappreciable even in the nicest operations of the physical sciences.

## MISCELLANEOUS ARTICLES.

## NATURAL HISTORY.

C. Guerfant, professor of natural history at Rouen, transmitted lately to C. Cuvier, for the purpose of examination, a quantity of bones found in the rocks in the environs of Honfleur, by the late Abbé Bachelet, and which belonged to the cabinet of the central school of Rouen. C. Cuvier has discovered among these bones those of a kind of crocodile, hitherto unknown, and very different from the fossil animal of Maestricht, which some consider as a crocodile also. The jaw-bones of this crocodile of Honfleur resemble in their prolongation those of the cavial, only the teeth are not so equal, and the futures of the bones are differently figured. The most striking difference is in the vertebræ; those of all the known crocodiles have the anterior face of their body concave, and the posterior convex; in that of Honfleur it is precisely the contrary. The apophyses of these vertebræ are also more complex than in the ordinary crocodiles.

This animal appears to be about 18 feet in length; its bones are petrified, and strike fire with steel. Their hollow parts are filled with pyrites. They were found in a marley kind of stone, very hard, of a grayish colour, and from which they could not be disengaged without difficulty. Besides the bones of this crocodile, C. Cuvier has found others which seem to arise from small animals of the cetaceous kind.

## MEDICINE.

The following account of a very singular disease which attacks men and animals in the province of Chiacas, in the government of Potosi, is extracted from the journal of Lima: "The malady, which Dr. Como Rueno calls *furiosa locura*, is one of the most remarkable facts in the history of this province. It is chiefly in the village of Tutari that it attacks men and animals, but among the latter those (such as the ox, horse, sheep, &c.) which have been brought from Europe; for the vigogne, the guanaco, and other quadrupeds of the country, are not exposed to it. No force can restrain



a person attacked by this disease, during the first fits of phrensy. Being totally a stranger to sentiments of shame, he escapes from bed, and runs with violence to the mountains. He flies from precipice to precipice, and at last throws himself from the first steep rock he finds in his way. In general the unfortunate wretch is dashed to pieces; but if, by some uncommon chance, the fall does not prove mortal, he soon recovers his reason and health, and has nothing more to fear from a return of this fatal malady. I shall not attempt, says the author, to inquire whether the mineral effluvia which rise from the bosom of the earth in a country exposed to volcanic convulsions, may not have as great a share in this phaenomenon as the constitution of the inhabitants; but what is certain is, that it often makes its appearance in the province. This fact, adds he, has so much analogy with what we read in Ovid's *Metamorphoses* respecting the leap of Leucade, that the one would seem to have served as the original of the other. Who knows, says he, whether the antient fable may not have originated from some malady similar to the above?

A surgeon of Madrid has been able to dissolve camphor in water by means of the carbonic acid. This camphorated solution, injected into the urethra of persons afflicted with the stone, allays the pain almost instantaneously.

#### CHEMISTRY.

Mr. Hahneman, of Altona, has discovered a new fixed alkali, which he calls *pneum*, because, when heated to redness, its volume is extended to twenty times its usual size. It crystallises in large prismatic hexaedral crystals terminated by two inclined faces, one of which appears to be triedral, and the other pentaedral. These crystals neither run *per deliquium* nor effloresce; but when pulverised they dissolve at 300° of Fahrenheit in half their weight of water, and melt almost in their water of crystallisation. At 65° of the same thermometer, 140 parts require for their solution about 500 parts of water. By cold they separate from the water. They do not dissolve in alcohol. This alkali produces but little effervescence with concentrated acids. With vitriolic acid it forms



forms a salt not soluble in alcohol, and with difficulty in water. The neutral salts formed with this alkali, and the nitrous, muriatic, and phosphoric acids, and those in particular formed with the acetic acid, dissolve with ease not only in water but in alcohol. Its muriate crystallises in the form of feathers: its phosphate has a bitter taste. All these salts, except those formed by the phosphoric acid, part with their acids by heat, and the alkali remains pure: the sulphate requires for that purpose a red heat; the nitrate only 300° of Fahrenheit: it does not detonate on ignited coals; nor does it decrepitate, or become luminous, when thrown on them.

It is difficult to saturate this alkali with carbonic acid, as it lets it escape in the usual temperature of the atmosphere: when saturated with it, it assumes the form of a light earthy salt.

This alkali exercises an action on vegetable colours. It precipitates metals and earths from their solutions in acids, and in the same manner as the other alkalies. It produces no change on *mercurius dulcis*, but it gives to corrosive sublimate the colour of carmine. It precipitates the nitrate of mercury black. When combined with oils, it forms a soap which dissolves in alcohol. Lime, precipitated by it from muriate of lime, is soluble in distilled water. It does not decompose muriate of ammonia but at a heat of 100° of Fahrenheit.

#### METEOROLOGY.

During the violent storm on Sunday the 9th of November last, the mercury in a barometer at Walthamstow, Essex, was very visibly agitated; the vibrations up and down were perhaps about four hundredth parts of an inch. This vibration was observed by several persons, and was seen at different periods of the day, during which the mercury rose very considerably.

The same circumstance was observed in other places.

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THE  
PHILOSOPHICAL MAGAZINE.

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JANUARY 1801.

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I. *An Essay to illustrate the Principles of Composition as connected with Landscape Painting.* By Mr. EDWARD DAYES.

*To the Editor of the Philosophical Magazine.*

SIR,

AS landscape painting is at present a very fashionable pursuit, the following essay on composition may not prove unacceptable to some of your numerous readers; and should such further observations on the arts as I may have leisure to offer, be deemed of importance enough to find a place in your valuable Magazine, they shall be at your service.

I am, with much respect,

Sir,

Your obliged humble Servant,  
EDWARD DAYES.

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O! attend,  
Whoe'er thou art whom those delights can touch,  
Whose candid bosom the refining love  
Of Nature warms,  
And I will guide thee to her fav'rite walks,  
And teach thy solitude her-voice to hear,  
And point her loveliest features to thy view.      AKENSIDE.

*General Observations.*

IT will be found on inquiry, that the principles that govern one part of the arts extend to every other, whether the sub-

ject be landscape or history: this much it may be necessary to premise, as it indicates the dependence of one part on another as to the forming a whole. By an inquiry into the obstructions to the obtaining a knowledge of this elegant and pleasing part of the arts, we shall find it arise (as in most other cases) from want of knowing what plan to pursue, and often from improper instructions; much of our success depending on being once in a right road.

Many are taught to believe that by copying parts they will be able to form a whole; and this error is, in some measure, encouraged by many of our publications. Few things are likely to prove so injurious as pursuing petty plans; to dare much is the character of genius; and, if we must fall, let us, at any rate, fall like Phaëton.

The means most likely to enable us to acquire a knowledge in the arts is, first to study pictures, and then resort to Nature; remembering to add to the stock we may collect from the wisdom of others, such original matter as may result from our own diligence. But though copying pictures may be necessary, very little knowledge will be obtained by it, other than what depends on the mechanical parts of the art. He will at all times copy best, who paints best; nor can we hope ever to become great by merely imitating another: by such a practice we may learn how to mix colours, but that is very different from a knowledge of colouring. It is true, we must reason from cause to effect; but that is a mode of inquiry seldom pursued by the mere copyist.

Composition embraces two considerations: first, as it respects alterations which may take place in a view, and which is by the artist termed composing it; and, secondly, as it applies to works of fancy purely. But as the principles of one regulate the other, all that will be necessary is briefly to state that no license should be taken with the view so as to affect the general features: diversifying masses of earth, agreeably breaking the foreground, or the accidental introduction or omission of any inferior object, is allowable. The forms of mountains, should they appear disagreeable, may be thrown in shade, or involved in clouds, in part, to conceal them; and the shadows may be artfully introduced to produce an agreeable



agreeably-shaped mafs of light, though the objects themselves are unpleafant.

Though we may be thus confined in treating a view, there will be ample latitude for the difplay of our tafte in the formation of the clouds, trees, light, and fhade, and in the difpofition of the animated objects. One thing highly neceffary in the introduction of figures is, that they enter into and make part of the fcene; and not come in as mere accompaniments, or as having no connection with the reft of the picture. This error is daily practifed, and argues a moft futile imagination. A man and woman talking, a folitary failor with a bundle at his back, or miferable fifherman, with now and then a cow or two to keep each other in countenance, form the utmoft ftretch of fome people's fancy. By a little reflection we fhall avoid fuch abfurdities, and be enabled to introduce our little group with fitnefs. As all ranks of perfons inhabit the country, it admits of the utmoft diversity in the figures; any degree of elevation or delicacy may be given, if accompanied by an appropriate employ. If the fubject is pastoral, though the figures need not be Arcadian, the low and vulgar fhould be carefully avoided: it is our duty to raife, not deprefs, the human fpecies: though our purfuits are humble, they need not be mean.

One thing neceffary to the acquirement of excellence in this (or, indeed, any other branch of ftudy) is, to think it an object of fufficient confequence to deferve all our attention: this will prevent our falling into a carelefs habit, and, of courfe, going from bad to worfe. Lord Chefterfield's obfervation fhould never be loft fight of—"What is worth doing, is worth doing well." To think meanly of the arts, is to want the means to become excellent. Let us guard againft a common error, that genius cannot exift unconnected with diffipation: the fact is, the moft renowned artists have been the moft temperate. Intemperance and ftudy cannot exift in the fame mind, or at leaft in fuch a degree as to produce any fenfible advantage. He who begins his career of life in the gratification of his corporeal pleafures, will in time find the memory of all other delights deadened, and ultimately

sink into a torpor, from which it will be impossible to rouse himself.

When this the watchful wicked wizard saw,  
With sudden spring he leaped on them straight;  
And soon as touch'd by his unhallow'd paw,  
They found themselves within the curst gate,  
Full hard to be repass'd, like that of Fate. THOMSON.

The figures in the bustling scenes of Vernet are highly appropriate, and will be well worth consulting; and those in the landscapes of N. Poussin are excellent examples of the higher style. Gainsborough appears to be the only instance of the true pastoral that this country has produced, and is well worthy our attention for the figures.

### *Materials.*

In treating of composition as it relates to works of fancy, it will be found to involve an inquiry after proper objects, and putting them together so as to form a picture.

By proper objects is meant the most perfect of their kind, accompanied with an application the most judicious. And here begins one of our greatest difficulties, the discovering what is proper, as it embraces an extensive field of action: whoever has acquired a knowledge of what is right, is in a fair way to do what is right. The foundation of all taste is general inquiry, or an inquiry after the species; for, though all trees are green, and those of the same genus resemble each other, and though rocks and mountains bear a particular form, yet some are confessedly superior, and should be carefully selected; for *painting is not the art of imitating Nature merely*, but requires the aid of reason in choosing the most perfect of her works, and rejecting her deformities.

He that brings fulsome objects to my view  
(As many old have done, and many new)  
With nauseous images my fancy fills,  
And all go down like oxymel of squills.

ROSCOMMON.

This principle of general nature equally extends to colour and every other part of the art, a knowledge of which can  
only



only be obtained from Nature through the medium of art; that is, by carefully attending to the different excellencies found in the works of the most esteemed masters, and diligently comparing them with Nature. By such a practice we shall in time be enabled to feel their beauties, and then we may consider ourselves in a fair way to possess the like. It is not a slight or superficial view of an esteemed picture that can benefit us; we must fix our mind steadily on it till we have, as it were, analysed it, or discovered the cause of each particular effect, as the only means to enable us, in our future operations, to work on similar principles.

With a view to assist our inquiry, it may not be amiss to point out the peculiar excellencies to be found in the works of some of the most celebrated landscape painters. N. Poussin, for dignity, will be found highly deserving attention; his buildings and figures are unequalled. Titian's colouring is rich, accompanied with great freedom of hand and fine forms of trees. G. Poussin's mountains are grand, and generally form a fine line of horizon, with a peculiarity in the deep parts of the picture, and depth of water truly grand. The eccentricities of Salvator Rosa will furnish an example of great union of parts as to chiaro-scuro, colour, composition, and figures, while his rocks are sublime and grandly formed; the whole accompanied with great freedom of penciling. The colouring of Claude is fine, accompanied with a lovely tone of air; but his compositions often appear studied; or, in other words, over-laboured, from the introduction of too many beautiful parts. Wilson's compositions are grand, with a tone of colour truly *Titianesque*, and a light and shade unequalled.

The pastoral excellencies must be sought among the works of the Dutch and Flemish masters, particularly in those of Rubens, whose colour and chiaro-scuro are fine; Teniers jun. who, for firmness of touch and the true silvery tone, stands unrivalled; Cuyp, for a light elegant touch, fine colour, and cattle; P. Potter for cattle, and, in his best pictures, a deep tone of colour; both the latter for a light elegant pencil, exquisite warm amber colour, and elegantly formed trees; Beughem and Wouverman's for animals, with a long string of et-cæteras:

cæteras: Canaletti's buildings are boldly handled, with a fine tone of colour. To enumerate the excellencies found in the different masters were endless; these hints may serve to direct our inquiry.

It would be unpardonable to pass over the merits of our countrymen Gainsborough and Barrett (not to mention many justly celebrated living artists, whose works will ever rank among those of the first masters). The former, for lightness of handling, elegant rusticity of figures, breadth of *chiaro-scuro*, and sweet silvery tone of colour, is highly worthy of attention; while the latter, for the character of a tree and the true tone of gray distance, is highly estimable. The three great names of Wilson, Gainsborough, and Barrett, form a school for the student, while their labours reflect the highest honour on our nation.

Some author calls painting a *sixth sense*; it certainly adds a delight to the existence of the artist, by enabling him to enjoy many beauties unnoticed by the common eye.

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For him the Spring  
 Distills her dews, and from the silken gem  
 Its lucid leaves unfolds: for him the hand  
 Of Autumn tinges every fertile branch  
 With blooming gold. AENSIDE.

Those beauties, as they escape common observation, so it will become our bounden duty not to suffer them to pass without making such memorandums as may secure them for future use: unless we do this, and resort frequently to Nature for our materials, we shall fall into a habit of repeating ourselves, and our pictures will appear as if made up from the same small stock of ideas. It becomes an indispensable duty with us to view every thing with an eye to the art; from the palace to the cottage, from the craggy rock to the humble bank of earth: the various kinds of trees, with each species of shrub, must not pass unnoticed. To the artist every thing is of use; beautiful or terrific, awfully vast, or elegantly little; all, all must be treasured up for future use: but still in our research, not the individual, but the species, must form the object of our inquiry.



The higher style of landscape is by some termed the *heroic*, to distinguish it from the pastoral \*. Among the objects of which the former is composed may be considered temples, pyramids, ruins of antient palaces and castles, altars, &c.; mountains covered with snow or involved in clouds, hanging rocks, and huge blocks bursting, as it were, out of the earth, &c. &c. Of objects for the latter, cottages, close woods, with open views of champaign countries, &c. &c. may be noticed. These can only be considered as making the general features; a further information will depend on industry.

To act is as necessary as to think: he who spends a life in comparing the styles of different masters, their peculiarities of colour, effect, or the propriety of their compositions, may in the end find himself a mere critic, but will never raise himself to the rank of an artist. Great advantage will be derived by frequently comparing our works with the labours of others, which will give us cause to rejoice at our improvement; or, what is a great step towards it, discover our defects.

#### *Combination.*

In forming compositions it will not be sufficient to bring together materials only; this can, at best, serve only to indicate a fertility of invention; they must be combined in such a way as to preserve an unity in the whole. Imagination is shown in the production of materials, but to arrange them requires the soundest judgment. To make all the parts of the picture tend to excite but one emotion will require the utmost care. If the scene to be described is solemn, no lively or fantastic image can be admitted: on the contrary, if the agreeable is intended, every thing gloomy or sad should be rejected. The necessity of this union of parts is equally understood by the poet and painter, as the following quotations from Milton will evince: there is the utmost unity of parts in each, though tending to produce different sensations.

Right against the eastern gate,  
Where the great sun begins his state,  
Ro'ld in flames and amber light,  
The clouds in thousand liveries dight,

\* There is a third, a sort of mixed style, which does not deserve particularising, as it is composed of the other two.

While the ploughman, near at hand,  
 Whistles o'er the furrow'd land,  
 And the milkmaid singeth blythe,  
 And the mower whets his scythe,  
 And every shepherd tells his tale  
 Under the hawthorn in the dale. L'ALLEGRO.

How animated, how lively is the whole effect, particularly when contrasted with the following :

Till civil suited Morn appear,  
 Not trick'd and frounc'd as she was wont  
 With the Attic boy to hunt,  
 But kerchief'd in a comely cloud,  
 While rocking winds are piping loud,  
 Or usher'd with a shower still,  
 When the gulf has blown his fill,  
 Ending on the rustling leaves,  
 With minute drops from off the eaves. IL PENSEROSO.

It is evident from the above examples that figures, colour, and *chiaro-scuro*, must all have one tendency, or the picture can never form a complete whole.

We need not wonder at that want of information in the higher walks of art which at present pervades society, if we consider the want of knowledge in those who make a trade of teaching, and that of the number of drawing-books poured on the public. Some consider neatness an object; others, touch, the form of a tree; or usher forth, as examples of art, incorrect sketches to copy; all being content with offering a part, none teaching the combination of a whole; or that to embody a grand idea is the highest point of human intellect. We are in duty bound to exert ourselves to improve the national taste by every fair and honest means; and, should we be so unfortunate as not to succeed, we may be comforted by the recollection that to labour to obtain excellence is excellent, as well as to endure whatever may happen to be the result.

Many deny the utility of the arts, while others acknowledge them as remotely useful only; but this were to question whether sight be preferable to blindness, sense to folly, or life to death. As we exist in the senses, to give them a  
 keener



keener relish through the medium of the sciences, is truly Epicurean.

The best method to improve and elevate our thoughts will be, by frequently contemplating the most noble objects in nature, and taking every opportunity of viewing pictures the most likely to inspire fine ideas. But we shall view fine works of art to very little purpose, if we feel no higher wish than mere imitation: if the noble enthusiasm of rivalry does not possess our breasts, it is much to be feared our remarks will be cold, and our exertions languid: let us then, like Jacob, dare to wrestle even with an angel.

Various schemes have been recommended to assist the powers of imagination. One advises contemplating the breaks in the plastering of old walls; another, the veins of marble; and a third, as the *ne plus ultra*, has produced a system of blotting: but those methods, however ingenious, are fallacious. Gainsborough is said to have formed landscapes on the table with broken stones, dried herbs, and pieces of looking-glass; and Chatelet, whose drawing he was fond of, used to design his rocks from lumps of coal.

This is a bad practice. If we do not resort to nature for our materials, and connect our inquiry with the best works of art, our compositions must not be expected to rise above the pastoral character of those of the former, and our rocks, like those of the latter, may smell of the coal-hole.

The Dutch painters, in their local representation, have copied each object as it occurred, without attempting to improve them; which many imagine gives them a certain natural air, which, by the bye, is absurd, and argues a great want of taste: this error equally attaches to the Flemish school, and may be seen even in the landscapes of Rubens. The universal affection for landscape painting does not arise from the love of imitation merely; the pastoral scenes of the Dutch delight from other motives, and principally because familiar to every imagination; they exhibit a life of peace, leisure, and innocence, with joy, plenty, and contentment; blessings not to be found in the bustling scenes of active life. One rule we are bound to observe in the pastoral; that is, not to represent scenes of wretchedness, or such objects as

may disgust. In composing scenes of rural life, though they do not require any great elevation of thought, or extraordinary capacity in the arrangement of the parts, yet they demand the greatest care in the finishing, cleanness and delicacy in the colouring, and the utmost unity and simplicity throughout the whole. To give interest, we should add all that stock of lesser graces dependent on ourselves; such as a light elegant touch, beauty and cleanness of colour, and a graceful simplicity of form. Scenes that do not interest from themselves must be made to do so by the labours of the artist; but where the subject is grand, we should clothe it with all the dignity of art, accompanied with a broad, firm, and spirited handling.

The beautiful, in painting, as in poetry and music, is calculated to move the softer passions; therefore every thing abrupt and hard should be rejected in the forms of the objects, *chiaro-scuro*, or colour; as is instanced in Claude's best pictures. But where a stronger emotion is to be raised, the images, forms, colour, and light and shade, by possessing contrary properties, will conspire to excite opposite sensations, as in the works of Salvator Rosa, and some of Wilson's grand compositions.

Abrupt, angular, and dark objects associate best with the sublime, accompanied with a certain degree of obscurity and depth of colour.

Violent passions of the mind are ever accompanied with actions more or less angular; on the contrary, beauty loves the easy sweeping line of grace, with perspicuity, light, and a richness of colour: in fact, one effect should, as much as possible, be calculated to excite painful, the other agreeable, sensations. Mountains involved in clouds, and objects seen through a mist, will always appear with more dignity than if distinctly viewed.

All agreeable sensations are founded in temperance: too great a quantity of light, colour, or sound, excites pain. It is the temperate of eating, drinking, sleeping, nay, of every thing, that makes them delightful. The intemperance of Alexander caused him to weep for more worlds to conquer—Horrid!

Massy and dark skies will best associate with the grand,  
while



while the more light and fleecy will ever attend the beautiful.

As lines are strongly expressive of motion and passion, perhaps in a boisterous sky the forms ought to intersect each other more angularly than in a tranquil scene; for, as a straight line is indicative of rest, every departure from it must imply motion; therefore the nearer the forms, in crossing each other, approach to a right angle, the more expressive of violence. But we must use caution in the practice. The slowly gliding river excites the idea of rest in its straight lines, in opposition to the contorted ones in the rushing current: the violent motion of lightning is always in acute angles. It is nearly the same whether the eye or the object moves: if the sight is employed in tracing lines running abruptly in contrary directions, it will convey the idea of violent motion, though it is not the lines but the eye that moves. The easy serpentine sweeping lines, expressive of the meandering of rivers or roads, serve wonderfully to carry off the distance. Of this there is a fine example in the picture of "Going to market," by Rubens, at Buckingham-house. The same thing, if used with caution, in the formation of the clouds, will carry the eye into the distance, and help the deception.

Those who have not the opportunity of frequently applying to pictures, will find their advantage in a good collection of prints; but they must not be used to steal from (which is beneath the dignity of a great and independent spirit), but to study from, to acquire a knowledge of composition and *chiaro-scuro*.

An indifferent original composition will always be superior to one patched together with stolen materials. The arts would be unworthy our pursuit, were they of easy attainment; we should therefore (if we wish to attain excellence) be careful not to suffer ourselves to be robbed of our time through an indolence of spirit; something should be done every day, if we expect success. He who feels a desire to act, should be prompt to embrace the golden offer: if we neglect the opportunity to-day, we shall feel less inclined to-morrow; nor are we sure at any other time we shall be able to rekindle the same ardour. To act is far easier than to

suffer; let us therefore husband that time, the continuance of which is so uncertain, and whose loss is irrecoverable.

Be wise to-day; 'tis madness to defer;

Next day the fatal precedent will plead:

Thus on, till wisdom is push'd out of life. YOUNG.

In arranging the materials of our picture, all that can be recommended is, to avoid regular forms in the mass of objects; as angular, circular, or any geometrical figure. That regularity which constitutes a beauty in buildings, would become a deformity in landscape. Should a long line of horizon, or any other objects, occur in a view, to diversify it, some part should be left obscure, if it does not admit of breaking; and in composing the clouds, and light and shade, we have an opportunity of helping the effect by giving them a slight tendency to a contrary direction: few things appear so unpleasant, as tiresome long horizontal lines following each other. But, however desirable variety may be, we should be careful to guard against running into an affected contrast; a worse evil of the two. We must be cautious that our composition does not crowd too much into one part of the picture; but, by way of supporting a sort of balance, some one mass, as a counterpoise, should appear in another part: this is not confined to the objects merely, but equally extends to *chiaro-scuro* and colour, to prevent a spottiness. It is a fault not uncommon to have too many parts in the composition: this should be avoided in elevated scenes, whose parts should be simple and little decorated: much finishing would destroy the simple dignity such subjects require. The back-grounds to some of Sir Joshua's pictures are in the first style of landscape painting.

It will be our duty to divest ourselves of prejudice as much as possible in viewing works of art; if we become partial to one master, we lose the benefit we ought to derive from them all: and let us by no means adopt the conduct of those who view old pictures to find out their excellencies, and modern ones their defects. Painting, as before observed, is not, what many ignorantly suppose, the art of copying Nature merely: no, no; it requires the aid of reason, and strong  
reason,



reason, to judge of what is fit for the art, and that it is which makes it an art indeed: a trifling skill will enable a man to become a mere imitator. It must be observed, however, that if our composition does not rise above common nature, it will be less interesting than a more indifferent local scene, fitted to increase our topographical knowledge.

From the whole of our inquiry it results, that the mind should never cease from its pursuit after whatever is beautiful or grand; let us then, by an abstract inquiry, endeavour to create a nature of our own, if possible, more dignified and noble than the one that strikes our senses: we should feel an enthusiasm in our pursuits not to be satisfied with any perfection short of divine.

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II. *Letter from Professor DE CARRO to Dr. PEARSON, on the Vaccine Inoculation.*

DEAR SIR,

Vienna, Oct. 22, 1800.

I CANNOT wait any longer to communicate to you my further success in the vaccine inoculation. A dreadful epidemic small-pox has given this autumn a new lustre to that practice, which, from the sensation it has created in this city, is not likely to be ever laid aside.

This extension of practice, which hitherto has been entirely, or nearly so, in my hands, has enabled me to estimate the accuracy of your observations on the inefficacy of a cow-pock that comes too rapidly to its height, and which does not follow the laws of that disease. I must candidly confess, that until this autumn I had not a very exact idea of the vaccine pustule; that is to say, I knew very well how it should be to correspond with the description of the English inoculation, such as I had seen it on my own children; but I was not aware that it must be absolutely so, and that every appearance after inoculation was not to be looked upon as a true vaccine. The astonishing likeness of my vaccine pustule in my sixty last inoculations has given me a true knowledge of it, and I scarcely believe that I can in future be mistaken.

On

On the third or fourth day, one perceives a slight elevation and redness; it increases in a vesicular form till the 12th or 13th day, and contains always the most limpid fluid: a fever comes on the eighth or ninth day, or sometimes is not perceptible: the beautiful areola appears on the seventh day, and increases till the crust is entirely formed: this crust begins commonly on the twelfth day, and becomes quite black.

I have had some cases originating from count Mottet's matter, where a patient had received the small-pox by inoculation, and three by natural contagion: one of these has retaken the vaccine in its perfect form. Luckily for the success of the inoculation, the parents of these children were advised previously of the idea I had of the imperfection of their former vaccina; and they were not surprised at it, after having had a great difficulty in believing that the superficial crust that it had formed would be sufficient to preserve them from future infection.

I have had two cases where the children were already infected with the small-pox at the time of the vaccine inoculation, and where it showed itself, as usual, without interrupting in any degree the course of the cow-pock. One of them proved excessively mild; the other is still dubious.

I have had two cases where the inoculated spot gave signs of inflammation only on the twelfth day, and where the pustule has been perfectly regular and complete. That was the same subject that had been seven months ago inoculated with the imperfect cow-pock.

I have not yet seen any pustule either of the *fugitive* or of the *variolous*-like appearance, although, according to Dr. Woodville's observations, they should appear more commonly where the small-pox is epidemic. There never was, perhaps, so disastrous an epidemic at Vienna as that we have now. It is certainly owing to it that people think so much now of the cow-pock, against which they have been exceedingly incredulous. I hope to have it in my power to continue it all winter. By the last accounts I have of the progress of the vaccine inoculation at Geneva, it is exceedingly rapid. Some eruptive cases have lately been observed there; but I need not enter into these particulars, as you will have them immediately



immediately from Doctors Petchier and Odier, or from the *Bibliothèque Britannique*. I have had lately two opportunities to make experiments upon persons who have *undoubtedly* had the small-pox in their infancy. They are both medical men, and consequently their testimony is more admissible than that of most others. Upon the one, the vaccine matter produced no effect whatever; and upon the other, a very small superficial vesicle appeared, which changed soon into a crust. In one word, there was not the least appearance of the true and usual cow-pock pustule. They confirm *your principle, that one cannot have the cow-pock after the small-pox*. One single circumstance vexes me in my vaccine practice: it is the frequency of inoculations that fail; that is to say, where no pustule nor inflammation is produced. I can scarcely attribute it to my method, which is the common puncture of most practitioners. I make it as superficial as I can, to avoid, as much as possible, to draw blood, which I believe can wash off the matter. I make one puncture on each arm. I see that in England, as well as abroad, inoculators are divided upon the advantages of the puncture or of the incision. You and Dr. Jenner seem to make use of the puncture: Dr. Woodville recommends strongly the incision. At Geneva they prefer the incision; and here the puncture. I propose now to make comparative trials; and to inoculate my first twenty patients in one arm with the puncture, and in the other with the incision. The difficulty of making the most superficial incision without drawing blood, has hitherto induced me to recommend the puncture. Upon the whole, I dislike much to be so often under the necessity of repeating the inoculation. You will oblige me very much by favouring me with some particulars upon this point.

My second patient, where the two diseases have gone on together, has had also a very favourable small-pox. Two similar cases in England authorise you to believe that the cow-pock can render the small-pox milder when they attack the same subject together.

The medical world is waiting with impatience for the work you have promised in your *Statement*; and I join my request to those that must already have been addressed to you  
upon

upon the publication of a work that must throw the greatest light on a subject which engages now the attention of all civilised nations. Whenever my materials shall be sufficient, I purpose writing a complete treatise on the subject for the use of the continental physicians, and such persons as are not in the way of collecting from the various sources of information which a little zeal and activity have opened to me.

I have the honour to be,

Dear Sir,

With the sincerest esteem,

Your most obedient humble servant,

J. DE CARRO.

P. S. I sent lately some vaccine matter to Lord Elgin at Constantinople for the inoculation of his only son.

III. *Letter from JOHN BRANSON, Esq. Surgeon, to Dr. PEARSON, on the Vaccine Inoculation.*

DEAR SIR,

Doncaster, Nov. 23, 1800.

**I**N my last letter to you I mentioned my ideas respecting the insusceptibility of the vaccine contagion to a person who had previously had the small-pox. I am still of the same opinion, as I have never yet been able to produce the true vaccine pustule in a person who had before undergone the small-pox; but I have lately had a case from which it would appear that, if both diseases are introduced into the system about the same time, they will both be received, and proceed through their regular stages, without being at all affected by each other. A child was inoculated for the cow-pock, after it had been for some days exposed to the contagion of the natural small-pox, on Friday the 26th of September. The arm went on in the usual way till Wednesday evening following, when the child was seized with fever, and in the evening of the next day the small-pox appeared. The eruption was confluent, and particularly so about the inoculated part, on Monday the 6th of October, which was the eleventh day from the inoculation of the vaccine matter, and the fifth from the commencement of the fever of the small-pox. I

8

inoculated



inoculated a child from the cow-pock pustule surrounded by a cluster of small-pox; in short, they appeared so blended that I had my doubts whether the matter I obtained was that of the cow-pock or mixed with small-pox; indeed I was rather disposed to think the latter was the case, as it did not appear so limpid as it usually does in the cow-pock. However, the result is, that it produced the genuine cow-pock without any appearance different from what I have always seen; and with matter from this source I have continued to inoculate ever since, without any variation either in symptoms or appearance. If this case furnishes you with any thing new on the subject, I shall be very glad. I have no time now to make any comment upon it, as the post-office is just going to shut. I am, Yours sincerely,

JOHN BRANSON.

P. S. Since I wrote last to you, I have inoculated upwards of 500 for the vaccine disease, without any unpleasant symptoms.

IV. *Report of C. NOWELL, M. D. of Boulogne, Correspondent of the Committee of Medicine, commissioned to repeat at Paris the Experiments respecting the Vaccine Inoculation, to C. MASELET, Sub-Præfect of the District of Boulogne-sur-Mer\*.*

YOU are desirous of knowing the results which I have obtained respecting the new inoculation known under the name of the *vaccine*. As it was you who first patronised it, when Dr. Woodville, Dr. Aubert, and myself, introduced it into France†; and as it was you who triumphed over that coalition of prejudices, interests, and passions, which had armed against it even the supreme authority, you ought to be more interested than any other person in the success and propagation of a discovery which, without your assistance, would have been rejected, together with the vessel that brought it to your fellow-citizens.

\* From *Decade Philosophique*.

† This is not true. The Vaccine Inoculation was introduced into France by the London Vaccine Institution.—See the Letter of the *Ecole Medicale* to Dr. Pearson, in our Magazine for August 1800, p. 279, &c.

Since the month of September, when I returned from Boulogne, I have inoculated for the vaccine 160 subjects of all ages, but the greater part children. All these different experiments, which were attended with the greatest success, have invariably displayed that character of mildness which establishes, in an incontestable manner, the superiority of the new over the old mode of inoculation; and the difference which we have had an opportunity of remarking in some of the results tends still further to confirm this superiority.

All the children and adults whom I inoculated during the whole course of the experiment continued their usual sports and exercises without feeling the least inconvenience. The adults followed their daily occupations, and in general experienced a very short sensation of uneasiness, and at most a slight febrile affection.

Six weeks after my first experiments I made successively seven counter-experiments. I carried to the house of C. Beaufoeil, whose children were attacked with the natural small-pox, the eruption of which was very abundant, three children of C. Beugné, who had been inoculated with the vaccine. After leaving them a long time together, I inoculated my three subjects, as usual, with the variolous matter taken from the youngest of the children, who was covered with the eruption; but none of them experienced the least effect from the operation.

The other four subjects, whom I afterwards subjected to a counter-experiment, were, a young domestic of C. Wynne, another of C. Bell, and two sisters of the latter. I carried them to the house of C. Trudin, broker, whose child had the natural small-pox. They were all inoculated without the least effect.

C. Magnier, of Pont-de-Brique, having sent for his son from Boulogne, where he was at school, I inoculated him with the vaccine in the country. He was then sent back to school, where the small-pox had broken out during his absence. The young man continued as usual to frequent the company of his school-fellows infected with the contagion, to play with them, to eat with them, and to sleep in the same room with them, without experiencing any effect.

I have



I have had an opportunity in the course of my experiments to be convinced that the vaccine inoculation can have no effect on those who have had the small-pox. C. Beugné, already mentioned, believed that he had never had the small-pox. I inoculated him three different times with the vaccine matter, but always without any effect. Being struck with this phænomenon, I asked him some questions, when he acknowledged, that at the age of four years he had slept with one of his sisters who had been attacked with the confluent small-pox highly malignant, that he had been very sick for several days, but that his mother did not think him infected with the small-pox because he had no pustules, which are the usual symptom. This was an excellent opportunity for trying a new sort of counter-experiment: I inoculated C. Beugné with the small-pox, but they took no effect.

The son of C. Genou, of Samer, aged nine years, had been inoculated for the vaccine with fresh matter and of a good quality. Four days after I went to examine the child, and was much surprised to observe that the vaccine had produced nothing, though the child had been inoculated in three different places. This experiment had seldom failed, except when the subject had before had the small-pox. On examining the child, I found him with a languid look, a bad pulse, and a great deal of fever. I suspected, therefore, that he had been already infected with the small-pox at the time when I inoculated him for the vaccine. On inquiring, I found that the child had come from a house where the natural small-pox prevailed, in order to be inoculated. I foresaw that he was going to have the natural eruption, and indeed the disease appeared on him two days after. It was of the mild kind, and the punctures made for the vaccine were dry, and almost effaced.

My experience proved to me also, that in the case where the subject is already infected with the small-pox, the vaccine inoculation, which then remains without any apparent effect, adds nothing to the malignity of the small-pox. I am convinced that, whichever of the two infections is anterior in point of action and progress, prevents the action and pro-

gress of the other; and consequently, as these two principles cannot exist together, the one cannot add to the other.

Another very important observation, which appears to me to prove that the vaccine is really a species of the small-pox, which differs from the human small-pox only by its origin and the peculiar character of mildness by which it is distinguished, is as follows:—A child of C. Billier, salt-refiner, on the ninth day of inoculation for the vaccine, which is the critical day, cut four teeth, two of them eye-teeth. A second crisis of nature so violent, ought to have been, and was accompanied with, a strong access of fever, which, added to the very moderate access arising from the vaccine, brought out on the child's body an eruption of 262 large pustules filled with limpid matter. No common inoculation ever produced any of so beautiful a kind; they ended without the least fever, and disappeared at the end of seven days without leaving any marks. This child since inoculation has enjoyed perfect health; and I have remarked that is invariably the effect of the vaccine. The case is far from being the same with the common inoculation. Of the children whom I inoculated with the vaccine, several were weak and ailing, often attacked with a violent cough, and two of them were wasted with a periodical fever. All these children, and in particular the two last, have enjoyed since the operation perfect health.

The vaccine inoculation serves me as a criterion to dissipate the uneasiness of those who have need of being assured that they have had the small-pox, and that they have nothing to fear.

I have offered a premium for every poor person who can be proved to me to have had the small-pox after being inoculated, with care, for the vaccine. I should not be afraid to risk my whole fortune at present on this head. But I cannot too strongly recommend to practitioners to bestow more care on this operation, and to repeat it if the least doubt should remain; otherwise there will be some danger of seeing persons attacked with the small-pox after being supposed secured from that disease by the vaccine. It is to errors of this kind that the obstacles opposed to the introduction



duction of the common inoculation in the north of England were to be ascribed.

This inestimable discovery has triumphed over all obstacles as well as over every prejudice in England. Sophisms have been refuted by facts; and it is contrary to the rules of good logic to reply to facts by hypotheses. The celebrated Doctor David, of Rotterdam, wrote to me in the month of October last, that the experiments which he repeated with the matter sent from Boulogne had been attended with the most complete success; and Dr. Jenner, the author of this noble discovery, informed him at that period, that more than 50,000 persons have been already inoculated with the vaccine in England; that a third of that number had been exposed to every test possible without the small-pox ever taking effect: and, in the last place, for five years, during which time he has been constantly employed in repeating the experiments, no one has ever yet refuted this theory, or weakened the conclusions he has drawn from it.

J. M. NOWELL, M. D.

Boulogne-sur-Mer,  
Dec. 3, 1800.

V. *A short View of the new Electrical Experiments performed by Dr. VAN MARUM.*

[Concluded from Page 193.]

7. **W**HEN the conductor at the tower of Siena was struck by lightning, several persons, besides the common electric light, clearly observed on it a regular train of light; and Beccaria gave to this phænomenon, which he endeavoured, without success, to imitate with his electric machine, the name of *divergent electricity*. Mr. Landriani requested Dr. Van Marum to try an experiment of the same kind with his large machine. Both these philosophers considered this divergent electricity as an effect of the resistance which metals oppose to the influx of the electric matter when they have too small a diameter, and throw off at the sides the matter not received. To ascertain this fact, an iron wire,  $\frac{1}{5}$  of an inch in diameter, was placed at such a distance from the

the conductor, that sparks were drawn from it almost without interruption. The operators now observed with pleasure that this small wire, though in complete connection with the conductor, was covered along its whole length with a continued stream of light, and that this light consisted of small rays, which issued from the wire on all sides. The smaller the wire, the broader was the stream of light. Wire of other metals, of the like diameter, exhibited the same phenomena.

8. *Experiments to try whether the evaporation of vegetables during their being electrified would be increased when the pots containing them were insulated and brought in connection with positive conductors.*—After being exposed to electricity for a quarter of an hour, the loss of weight by evaporation was found to be in some 1-4th, and in others 1-3d more than in the unelectric state. It is here possible, however, that there may be some deception; for, as the electric conductor, when not made exactly round, always emits a wind, so must the points of the leaves of plants, and consequently more evaporable matter must be carried off with it. In order to obtain something decisive on this subject, plants without leaves ought to be employed; but these evaporate very little.

9. *The influence of electricity on the sensitive plants.*—This was proved by Dr. Van Marum on the *Mimosa pudica*. He first exposed the plant to the sun, in order that its leaves might be more expanded, and then applied it at the distance of two feet from a conductor positively electrified, and then to one negatively electrified. No effects, however, were produced. Being placed on the conductor, the small leaves raised themselves up a little and expanded, when no sparks were drawn from the conductor; but as soon as sparks were drawn from the conductor, the small leaves again fell nearly in the same manner as the pendulum electrometer. After a few such changes they at length began to approach each other, to close themselves up, and to become totally shut. This result took place also in consequence of other kinds of shocks, and therefore is not to be ascribed exclusively to electricity. In other respects, the plants experienced no other change.



In experiments of the same kind made with the *bedysarum gyrans*, the electricity produced no acceleration nor retardation in the movement of the small leaves.

10. Electricity produced *variations in the state of the barometer*, but only in a small degree when the mercury had not been boiled, and when, of course, it still contained some moisture.

11. An experiment was undertaken at the request of Volta, *to ascertain whether the evaporation of liquors at the common temperature of the atmosphere would be increased by electricity*.—Doctor Van Marum first placed water, alcohol, sulphuric ether, accurately weighed in small porcelain cups, on a conductor, and at the same time similar quantities at some distance from the machine. After being exposed to electricity for half an hour, no signs of increased evaporation could be perceived. The case was the same in other experiments where the circumstances were changed. Volta proposed also two other experiments; one for the purpose of trying whether electrified air be more impregnated with water than other air, and, whether the atmosphere be rarefied by electricity.

12. *On the reduction of metallic oxides by electricity*.—As this was effected in the year 1785 with sparks drawn from a battery, Dr. Van Marum was desirous of trying whether it could be done also with sparks from a conductor; whether air was produced during each revival; and if so, of what kind. For this purpose he employed tubes like those already described in art. 5. The sparks were three inches in length. Red lead was almost immediately revived, and, within twenty minutes, there were produced about  $\frac{3}{4}$  cubic inch of gas, 1-3d of which was carbonic acid gas. The residuum showed in the eudiometer not so much decrease as atmospheric air. White lead, treated in the same manner, was reduced in a less degree; less air also was produced; but in other respects it appeared to be the same. Oxide of tin was not reduced, and, after being electrified half an hour, gave no air. The case was the same with the oxide of iron. Oxide of mercury prepared by heat was very soon reduced into the form of very small globules, of a black colour, adhering to

the sides of the vessel. The quantity of air produced, however, was so small, that it could not be examined.

13. *Experiments for examining the quality of the air in the ball in which the machine was worked.*—For this purpose Saussure's electrometer was employed, but, instead of a pointed piece of metal, the flame of a wax taper was employed to receive the electricity, according to Volta's experiment. The air in this hall was of moderate dryness; and it was observed that, for the space of five minutes, during which the electricity was continued, the whole air was electric, though the hall was 60 feet in length, 30 in breadth, and 40 in height. The balls separated from each other at the remotest places half an inch; the conductor was positively electrified, and the air of the hall was electrified in the same manner.

14. Volta entertained an idea that the strength of the conductor would be increased if its length were increased. Experience, however, proved the contrary. Sparks drawn from a conductor 60 feet in length and 4 inches in diameter, were five inches shorter than those drawn from a common conductor. The sparks seemed to be a little stronger, but they followed each other more slowly.

15. In repeating the *experiments on the communicating heat to bodies by electricity*, Dr. Van Marum conceived the idea of transmitting the sparks through semi-conductors, in order to give them more energy. For this purpose he placed a wooden rod, one inch in thickness and 11 inches in length, between the ball of the conductor and the conducting wire. The consequence was what he expected; for a rod of red fir, after being electrified three or four minutes, gave sensible signs of heat; and a thermometer, sunk into a hole made in it, rose in three minutes from 61 to 88 degrees, and, in five minutes, to 112 degrees. As the sparks, however, often penetrated under the surface of the rod, it at length split at the end, and continually threw out rays sideways, so that it imitated the effects of lightning.

16. *Phosphorus electrified in vacuo* produced a gas by which the column of mercury in half an hour fell four inches;



inches; after which it became stationary. In a darkened room, the electricity in vacuo appeared of a greenish-yellow colour. In the middle, where the electric stream was stronger, as well as at the surface of the phosphorus, the rays appeared of a very lively red colour. After the passage of the electric current, the light did not continue a moment; but the air produced retained its elasticity till the day following. As soon as a little atmospheric air was introduced to it, the whole space above the mercury appeared luminous. Phosphorous gas was in all probability produced. The quantity, however, was too small to be examined, though of the same kind as that described by Gengembre in the *Journal de Physique* for 1785.

17. *Experiments with a battery containing 550 square feet of coating.*—This battery consists of 100 jars, each twelve inches in diameter, and from 22 to 23 inches in height. They are coated to within four inches of the mouth, and stand at the distance of half an inch from each other in four boxes of equal size. The boxes are five inches from each other, and are connected at the top by four brass rods, and at the bottom by four plates of lead. In the middle jar stands a perpendicular tube, furnished with a ball six inches in diameter, and 24 holes, into which the tubes of the other jars, an inch in diameter, are inserted: the lower ends of these tubes are inserted in the balls of each jar. In the jars themselves there are wooden rods placed on stands with peculiar supports, and on these brass rods are stuck, so that nothing is cemented to the jars. When this battery had been charged by 98 turns of the machine (100 turns made it discharge itself spontaneously, by which means the jar where the discharge took place was pierced through,) an iron wire,  $\frac{1}{40}$  inch in diameter and  $24\frac{1}{2}$  inches long, was thrown about to a great distance in small red-hot globules. By a comparison with former experiments of this kind, it appeared that the strength of the machine, by improvements made in the cushion, had been increased five-fold. Dr. Van Marum considers the fusing of an iron wire, of a certain length and thickness, as the surest means of ascertaining the strength of the shock of a battery. In one experiment, by a shock of the

above kind, a similar iron wire, 36 inches in length, was thoroughly ignited, so that it became blue, and soon oxydated at the surface. On this occasion, a phenomenon never before observed took place. At the moment of charging, the whole surface was covered with an exceedingly lively light, that could even be observed in the open day, and which in darkness appeared to be an inch in diameter. The shock also was much stronger than usual.

A discharge on pieces of quartz rounded the corners and points very perceptibly, and even fused some parts of the stone.

18. *Experiments with the battery to ascertain the cause of death in those struck with lightning.*—This cause is generally ascribed to the irritability of the muscular fibre being destroyed. Now, as animals hitherto have never lost their lives immediately by electric shocks, but have been lamed or thrown into convulsions, it appeared doubtful whether an electric shock could actually deprive the muscular fibres of their irritability. For this reason Dr. Van Marum in his experiments employed eels, which, as is well known, even when cut into three, four, or six parts, and when deprived of the head, still retain signs of irritability. These eels were a foot and a half in length, and the shock was conveyed through the whole body. By these means they were instantly killed, and never moved afterwards. They were immediately skinned, and trial was made, by pinching, pricking, &c. whether any irritability remained; but no traces of any were perceptible even when pretty large sparks were drawn from these parts. The strongest salts were attended with as little effect.

When the shock was made to pass through individual parts, for example the head, these only lost their irritability, while the rest retained it. When the head was kept free from the shock, the remaining parts only were lamed. The same experiments were several times repeated on eels of  $3\frac{1}{2}$  feet in length, and with the same results. When the shock was made to pass through the upper and fore-part of the head of large eels, the under-jaw, as well as the muscles of the neck and belly, and even the lower part of the body, retained their irritability, while it was completely destroyed in the parts through



through which the shock passed. The same effects were produced in warm-blooded animals, for example rabbits, with much smaller batteries. Now, as, in consequence of such derangement, circulation of the blood can no longer take place, this circumstance, without all doubt, is the cause of the sudden death of those struck with lightning. If the shock does not pass through the large arteries the animal may still recover, provided the cerebellum and spinal marrow be not injured.

19. *The effects of such shocks from a battery on trees.*—Nairne, in the year 1773, made experiments of this kind on different plants, particularly myrtles and laurels. Dr. Van Marum, for his experiments, made choice of much more vigorous trees, such, for example, as the young stems of a common willow-tree, and at a period, *viz.* the middle of April, when the young branches usually shoot forth. Through two of these, eight feet in length, he conveyed shocks, the first through a space 15 inches in length, and two others through their top. After the experiments these trees were planted; but the parts through which the shocks had been conveyed sent forth no branches. The upper parts through which the shocks had passed, sent forth a few shoots for some days, but very slowly, and which soon after died. Those not electrified sent out branches like the other trees planted near them. The effects of electricity, therefore, are similar to those of lightning.

20. *Experiments on conductors.*—Dr. Van Marum had concluded from his former experiments that a rod of lead must be four times as large in diameter as one of iron to withstand lightning in the same manner. These dimensions, however, are sufficient for the strongest lightning. It appeared also that copper conductors are equal to those made of iron, even when their diameter is only one-half of that of iron ones. This appeared also from Brooks's experiments, who concluded that a slip of lead four inches in breadth, and so thick that a square foot of it amounts to eight pounds, cannot be hurt by lightning. The experiments made by means of the large battery respecting the conducting property of copper, gave very different results. As an iron wire

$\frac{1}{40}$  of an inch in diameter and 36 feet in length, was reduced to a state of ignition by a charge from 98 turns of the machine, Dr. Van Marum made a charge of the same kind to pass through a copper one  $\frac{1}{75}$  inch in diameter, and saw, with astonishment, that this wire was fused into small globules. Another copper wire,  $\frac{1}{80}$  inch in diameter, was broken in two places from a like charge; a third, of  $\frac{1}{55}$  inch, remained whole. The cause of this difference, after a close examination, was found to be in the difference of the purity of the copper. In the last experiments the wire was of common copper, but in the former they had been drawn from an ingot of purified copper, like that prepared to be mixed with gold. Now, as conductors are made of common copper, in determining their strength attention must be paid to the last experiments, and their diameter must amount to half that of iron conductors, in order to present an equal resistance to lightning. It is here supposed that the length is always the same. As it has been found that square iron rods of half an inch in thickness can withstand the strongest lightning, in regard to copper ones, it will be sufficient if each side be about four or five lines.

Dr. Van Marum fastened the wire, through which he made the shock to pass, on a piece of baked fir-lath, and found that it was burnt in some places where it had touched the bends of the wire. Another time he bound agaric of the oak round the wire, so that it lay fast; and the consequence was, that it took fire along its whole length. Conductors therefore which pass along fir, or wooden work, must be made somewhat thicker than would otherwise be necessary.

Patterfson has recommended plumbago for the summits of conductors, because it cannot be fused by lightning, but a charge of the large battery always reduced the strongest plumbago to powder. If conductors are to be made pointed, they ought to be furnished with several points, that, in case one of them should be fused, the rest may be in a state to conduct the lightning. But it appears from former experiments that pointed conductors do not deserve that preference over blunt ones which some have given to them.



21. *Continuation of the experiments on the oxydating of metals; on this occasion, the semi-metals.*—As the semi-metals could not be drawn out into fine wire, thin plates of them were employed; but this could be the case only with zinc and bismuth. After the explosion the metal was seen to rise up like a thick vapour, leaving traces of it on the paper; but it could not be converted into ignited globules. Purified antimony, pulverised and strewed in a line, was oxydated, and exhibited the same phænomena as zinc and bismuth; but a larger quantity was converted into powder before it could be oxydated. Some semi-metals were mixed with so much tin that they could be drawn out into wire  $\frac{1}{35}$  of an inch in diameter; for example,  $\frac{1}{3}$  zinc and  $\frac{2}{3}$  tin,  $\frac{1}{9}$  cobalt and  $\frac{8}{9}$  tin,  $\frac{1}{25}$  bismuth and  $\frac{24}{25}$  tin. In their oxydation, however, they exhibited nothing particular; they always rose in vapour, and left spots on the paper like the unmixed metals. A platina wire,  $\frac{1}{75}$  inch in diameter, made by Jeanety at Paris, exhibited the same phænomena in fusing as silver. The platina was strewed about in fine powder, which left on the paper traces almost similar to those of iron.

22. The so-called residuum after the explosion was, with a charge of  $5^{\circ}$ , double of what it was by a charge of  $15^{\circ}$ .

23. Nairne found that, with a battery of 50 square feet of coating, the jars were readily broken, when the charge was made to explode by too short a discharging-rod, and he considered one of five feet in length sufficient to prevent this accident. Dr. Van Marum found this length sufficient for 135 feet of coating, but not for 225 feet; and, for 550 feet, a discharging-rod of 18 feet in length was not sufficient, for even then the jars were sometimes broken. None of the jars however were hurt when the communication was not made through the strong iron wire of the battery, but through very fine metallic wires, through imperfect conductors, animals, and the like, where the stream found more resistance. In charging large batteries it is necessary that the discharging-rod should receive the stream from the middle of the battery, for, when it was received on one side, two jars broke in succession on the opposite side with a charge of  $20^{\circ}$ . Brooks says

says that the breaking of the jars may be prevented if paper coating be applied under that of the metal. Dr. Van Marum found this to be true, but observed at the same time that the coating was thereby weakened.

VI. *Account of some interesting Experiments, performed at the London Philosophical Society, respecting the Effects of Heat, excited by a Stream of Oxygen Gas thrown upon ignited Charcoal, on a Number of Gems and other refractory Substances submitted to its Action; with a Description of the Apparatus employed.*

[Concluded from Page 266.]

**I**N the experiments that have been described, no account was taken of the quantity of oxygen gas expended on each, as the objects the Society had chiefly in view were:

1. To fuse, if possible, some of those refractory bodies which had before resisted every effort hitherto made to bring them into a state of fusion;
2. To ascertain whether any or which of the gems would retain their original colour, transparency and hardness, after fusion, with the view of determining whether it would be possible by such a process to unite small stones or fragments into one mass possessing the properties of the native unfused gems;
3. To determine whether any other gems besides the diamond could be dissipated by a strong heat; and,
4. To observe what other effects might be produced by the powerful agents employed in the experiments.

The oxygen gas employed was obtained from oxyd of manganese by exposing it to a strong heat in an iron retort, and was received over water in a pneumatic apparatus. The orifices of the blow-pipes were about a sixteenth of an inch in diameter, and the gas was discharged under a pressure of about three pounds weight on the bell of the gasometer.

The view of the apparatus (Plate IX. fig. 1.) will convey a very correct idea of the gasometer, and of the arrangements made for the last experiment, *viz.* the one upon platina,



The tub ABCD, three feet high and sixteen inches and a half internal diameter, is made of mahogany, and rests on a low stand. A large glass receiver E, open at the bottom, nearly fills the diameter of the tub. This receiver is twenty-two inches in height from the lower edge to the crown, about sixteen inches internal diameter, and is capable of containing about ten gallons of gas: it ascends or descends in the tub, guided by the rod FG attached to its top, and passing through the cross-bar HI. In this cross-bar there is some simple wheel-work worked by the pulley at L (concealed in the cross-bar), and which, turning an index, tells the quantity of gas expended from time to time, when that circumstance is wished to be noted. The pulley at L is worked by the chain KL attached to the top of the receiver. From the pulley at L this chain passes to the pulley M, where it is wound up or let off at pleasure by means of weights put into the shell N, or removed thence to the horizontal plate O, which, by means of a socket, may be either put on or taken from the top of the rod FG. This contrivance is for the purpose of enabling the operator to command the pressure of any required column of water which the tub can contain, which is therefore made deeper than the receiver. To facilitate this, the pulley M has two grooves on its circumference, one of which receives the chain KL, and is equi-distant from the centre all round; the other, which receives the cord to which the scale N is attached, is not equi-distant from the pivot, but in a snail form, so adjusted as, with a given weight in the scale N, to counterpoise exactly the glass receiver E, whether wholly or partially immersed in the water in the tub; so that, when no weight is on the round plate O, the height of the water within and outside of the receiver will be on a level; in other words, no pressure will be exerted on the gas it contains. By then putting known weights on the plate O, the receiver is pressed down in the water so as to maintain an uniform pressure of any required column of water during the whole time of the process.

The gasometer is charged in the following manner:—Water is first poured into the tub ABCD (till full if necessary); the receiver of course ascends (no weight being then

on the plate O), filled with atmospheric air: at its greatest height its mouth is exactly on a level with the mouth P of the tube P, Q, R, S, bent at right angles at Q and R, and joined at the end S to the tube *ab*, each end of which is likewise bent at right angles and furnished with a stop-cock. The tube TU, fitted air-tight to the end *b* of the tube *ab*, can be taken off at T without deranging the end U. This being taken off, and the stop-cock *b* being opened, a communication is made between the air in the room and that in the receiver; which being now pressed down, either by the hand applied, or weights put on at O, discharges the air it contains at the opening at T; the place of which thus becomes supplied with water up to the top of the internal tube at P, which almost touches the upper part of the receiver. If the experiment require that the portion of atmospheric air contained in the tube PQRS should be also removed (as when oxygen and hydrogen are to be combined in the formation of water), all that is necessary is to pour water into the tube, through the opening T, till all the air be driven out of the tube, and its place be occupied by water up to the opening T. Fill also the tube TU with water. By this means the receiver and the tubes are entirely deprived of atmospheric air. To clear the tubes from water, that the oxygen gas (or any other, according to the intention of the experiment) may find no obstruction when the receiver is wished to be charged, a large tin flask, having a neck ground to fit the cock Q, and charged with oxygen gas, is joined to the cock Q, which must then be opened: the gas ascends from the flask, and the water in SR and PQ takes its place. The cock U must then be opened, to allow the water in the tube TU to descend into the cask V.

Oxygen gas is then to be transferred to the receiver from a cask V, previously filled with it. The cock *a* is kept shut, and *b* opened, and water is made to descend from the funnel W into the cask by turning the cock *c*. The water, of course, displaces an equal volume of gas in the cask, and forces it through the tubes UT, *ba*, and SRQP into the receiver E, displacing the water contained in it. While the gas is passing into the receiver, the funnel W must never be

allowed



allowed to run itself empty, otherwise the atmospheric air getting into the bottom pipe, a portion of it might be forced down with the next supply of water. The receiver being charged with gas, shut the cocks *c* and *b* for the present.

Should any experiment require more gas than the receiver will contain, the stream may be kept up direct from the cask by opening the communication UT *ba*, and taking care to keep the funnel W well filled with water. Another cask may be joined to the first by a mode of communication so obvious that we need not describe it; so that oxygen gas may easily be supplied for a length of time sufficient to melt down completely the whole internal arrangements of any furnace.

In none of the experiments, except the 32d, were the table-furnace and its appendages employed. In the first seven, a simple blow-pipe was joined to the tube of the gasometer, and the substance to be operated upon, laid on a piece of ignited charcoal, was exposed to the issuing stream of gas. It was observed in the course of these experiments that the substances seemed sometimes to be partially cooled, by the stream falling on them while the charcoal was moving about to expose the different parts round the stone to the action of the gas. To obviate this inconvenience, the double blow-pipe already mentioned was constructed, the nozles of which formed such an angle as to make the streams of blast cross each other. From this it is obvious, that the charcoal placed at any distance from the nozles short of that where the streams would cross, received two streams of gas; and that the nearer the charcoal was brought to the blow-pipe, the wider was the space between the centres of the spots on which the blast fell. By this means it became easy to expose the gems and other substances to the full action of the caloric liberated by the decomposition of the oxygen gas, without putting them in the way of the undecomposed streams.

This double blow-pipe is represented in fig. 2. The part A, which joins the tube of the gasometer, turns air-tight in a collar B, on the end of the tube BC. On BC are two brass boxes *d, e*, into which are fitted the tubes *f, g*, which also turn in sockets air-tight for the purpose of enabling the operator to move the blow-pipes *b, i*, nearer or

further from each other. The blow-pipes *b, i*, also turn at *k, l*, in the tubes *f, g*, to enable the operator to alter the direction of the streams, and make them fall on the charcoal *m*, at any angle he pleases. The double blow-pipe just described was that employed in all the experiments, from the 8th to the 31st inclusive.

VII. *Extract of a Letter from Dr. SAM. L. MITCHILL, Professor of Chemistry in Columbia College, to Mr. TILLOCH.*

SIR,

New-York, Dec. 3, 1800.

..... CONSIDERING all things, I really trust that the western world is doing its part in the philosophical work of the day. We have great and excellent opportunities of observing phænomena here, and some among us are diligent to let no opportunity be lost. The following points we think fully established :

1. There is proof as pointed as it needs to be, that the sickly seasons which afflict our cities are accompanied with, and occasioned by, noxious exhalations, *locally* produced either on board sea-vessels, or in stores, cellars, dwelling-houses, and sinks, in our towns.

2. When existing in ships, these noxious productions are not received on board in the ports of the West Indies, or other places beyond sea, as is vulgarly believed, but universally are produced within the sides of the vessel itself, from nastiness and corruption there.

3. Not only are the exhalations from our *corrupting fish, beef, and hides*, and from our *abominable privy-pits*, very injurious to health, and destructive of life, but these very vapours are so *acid* as to be smelled and tasted by the repackers of provisions.

4. In the alimentary canal of such persons as feed upon beef and fish, and, generally speaking, of *lean animal substance*, there is formed a *similar acid*, whose presence has been detected, and of a strength sufficient to curdle milk, to excoriate the fundament, and to effervesce with carbonats of alkaline salts.

5. The



5. The same *acid* product, or some modification of it, which, when volatilised and spread through the atmosphere, produces our *endemic* fevers (for our annual distempers are not *epidemics*), of the various grades from intermittent to *yellow* and *pestilential*, does, when engendered in the intestinal canal, stir up *dysentery* and its concomitant symptoms.

6. The excellency of alkaline remedies, especially the neutral salts, in which soda is combined with a weak acid, evinces the existence of an inflaming and corroding sourness, which being overcome either by alkalies *per os* or *per anum*, gives the patient great and speedy relief.

7. The application of the same mode of reasoning to *the human mouth*, which has been employed with respect to the stomach and bowels, will explain the generation and noxious effect of a *similar acid* among *the teeth* and around the gums, of their corrosion and destruction by it, and of the utility and importance of alkalies as dentifrices and sweeteners of the mouth.

8. An explanation is, on the same principles, given to the manner in which *human garments* grow *foul* and *pestilential*, the excretions lodged in them degenerating to *acidity* by *exposure to the air*, and thus becoming *fomites of infection*. This infection never possessing any *specific* quality, but merely being the acid offspring of common putrefaction. And on this depends the theory of *alkalies*, and *leys* and *soaps*, in destroying infection if present, or in preventing its formation, and their wholesome and purifying power in washing, scouring, and housekeeping.

9. Experiments lately made in the New-York hospital have proved to me that *foul* and *ill-conditioned ulcers*, especially of the *siphylitic* kind, contain an acid so considerable as in *three or four hours* to turn *litmus-paper* red. I have found *alkaline remedies* of admirable use in such surgical cases locally applied. We are thus possessed of a clue to explain much of the nature of *corroding malignant* and *infectious ulceration*; of the *manner of stopping it by alkalies*; and of the explanation of *hectic fever* from an absorption of this *acid virus*.

10. It is rendered plain of what materials *cities* ought to

be built and paved to be most healthy, to wit, the *calcareous*; and that a wise policy should introduce marble and limestone into general use.

II. The use of *pit-coal for fuel* has an additional recommendation. During combustion it affords much *ammoniac*; and this volatile alkali is capable of neutralising *abundance of septic acid*. Commonly, where the burning of coal is general, pestilential distempers are more rare than they use to be. But my paper fails me before I have finished my enumeration: I must therefore conclude, and leave the rest for a future communication, though not without assuring you that I remain yours, with much regard and respect,

To A. Tilloch, Esq.

SAM. L. MITCHILL.

VIII. *Extract of a Letter from Professor ABILDGAARD, Secretary to the Royal Society at Copenhagen, to C. HUZARD, Member of the French National Institute, on the Quantity of Carbon in the Blood\*.*

I SHALL give you the result of some experiments which I made and repeated to discover the quantity of carbon that exists in the blood, and which gave me less of that substance in the arterial than in the venous blood.

1st, A hundred parts of the venous blood of a horse, when dried in a moderate heat, gave 26 parts of a substance so dry that it could be pulverised.

2d, A hundred parts of arterial blood of the same horse gave 25 parts of dry substance.

3d, To alkalis, in the manner of Kirwan, an ounce of nitre by detonation (the ounce being 480 grains), required 192 grains of venous blood, and only 160 of arterial.

4th, An ounce of venous blood, after being dried and decomposed in a close vessel, yielded  $115\frac{1}{2}$  grains of charcoal.

5th, The same quantity of arterial blood gave only  $87\frac{1}{2}$  grains of charcoal.

6th, To decompose 480 grains of nitre, required 148 grains

\* From the *Annales de Chimie*, No. 106.



of charcoal of venous blood; and to decompose the same quantity of nitre, required 119 grains of charcoal of arterial blood. This experiment, indeed, was not very correct, because a very light part of the charcoal was dissipated like dust.

7th, I separated the red part of the blood from the serum and fibrous part, as completely as possible, by the means commonly employed; and after drying it, I tried it by nitre. To alkalise 480 grains of nitre, required 130 grains of this red part of the blood.

8th, To alkalise by detonation 480 grains of nitre, required 202 grains of the fibrous part separated from the serum; and yet with this part the nitre detonated more briskly than with the other parts of the blood.

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IX. *Analysis of the Honey-stone, or Mellite.* By  
C. VAUQUELIN\*.

THE analyses of this stone given by Abich and Lampadius are well known. The former obtained from 100 parts of it, 16 of carbonat of alumine, 4 of carbon, 3 of the oxide of iron, 40 of carbonic acid, 28 of the water of crystallisation having the smell of bitter almonds, and 5.5 of naphtha.

The latter had for result 86.4 of carbon †, 3.5 of petroleum, 2 of flex, and 3 of the water of crystallisation; which makes an enormous difference.

Mr. Abich, considering the incombustibility of the mellite, proposes to remove it from the class of combustibles, and to place it in that of the incombustibles. But Professor Klaproth, whose labours are entitled to the greatest confidence, informed me several months ago that he found this

\* *Annales de Chimie*, No. 107.

† If M. Lampadius operated on the same substance as that which M. Abich and I analysed, it is impossible that he should have obtained 86.4 of carbon; for, in 40 of carbonic acid and 4 of carbon, obtained by M. Abich, there was not a sufficiency to form 86 of carbon; and as it appears from my analysis that there is not more than 55 per cent. of real acid in honey-stone, it is evident that 86 of carbon cannot be extracted from it. M. Lampadius therefore must have operated in another manner, or did not employ heat sufficient to analyse the acid, if the substance he analysed was really honey-stone.

pretended stone to be composed of a peculiar vegetable acid united with alumine.

Professor Abildgaard, to whom I am indebted for many curious minerals from Norway, sent me a few weeks ago, by the hands of M. Mantey, a small quantity of mellite, a part of which he destined for the purpose of analysis, and I took the earliest opportunity of complying with his wishes in that respect.

#### *Description of the Mellite.*

This substance has a light yellow colour, on which account it has been called mellite, or honey-stone: it generally crystallises in octaedra, the angles of which are sometimes replaced by facets arising from laws of decrement, which have not attained to their limits. Its specific gravity is not considerable; according to Mr. Abich it is about 1.666. It is found in Thuringia in strata of fossil and bituminous wood.

#### *Chemical Characters of Mellite.*

I. When exposed to the action of heat in contact with the air this substance becomes white, and burns without becoming sensibly charred: it leaves as residuum a white matter, which produces a slight effervescence with acids. It has no sensible flavour, yet, if kept for some time on the tongue, it occasions a faint impression of acidity.

#### *Analysis.*

II. I took two grammes of mellite, reduced to powder, and mixed them with four grammes of saturated carbonat of potash dissolved in a sufficient quantity of water. As soon as the mixture was made it produced a pretty strong effervescence without the assistance of foreign heat; but to accelerate the decomposition of this substance, and render it more complete, I exposed it to a gentle heat on a sand bath.

The liquor, when filtrated after cooling, had a brownish colour, and left on the paper a brown matter, which when dried in the sun weighed nearly 0.8 of a gramme.

III. These 0.8 of brown matter, when calcined in a crucible, became white, and weighed no more than 0.33 of a gramme.



gramme. When mixed with sulphuric acid diluted with water, they produced a slight effervescence : the mixture was then evaporated to dryness.

According to what had been announced by Professor Klaproth, I expected that by the addition of water almost the whole of the above matter would be dissolved; but, on the contrary, the greater part remained under the form of a white powder.

The liquor having been evaporated till there remained no more than 3 or 4 grammes, I added to it a drop of the sulphat of potash, and obtained by spontaneous evaporation about 0.1 gramme of alum mixed with a little sulphat of lime.

I then examined the nature of the matter, which when treated with sulphuric acid had not dissolved in water. For this purpose I boiled it with a solution of the carbonat of potash, and, when filtrated and washed, I had the following result :

1st. The muriatic acid, diluted with two parts of water, attacked it, exciting a strong effervescence; but the solution did not become clear; on the contrary, it remained milky.

2d. The liquor, when filtrated, gave with ammonia a transparent precipitate, resembling that arising from alumine by the same means; but it was not entirely soluble in potash. The greater part however was dissolved by the potash, and exhibited all the characters of alumine; for, when combined with the sulphuric acid, it gave alum. The cause therefore why this substance did not remain combined with the sulphuric acid was, in all probability, its being too much heated towards the end of the desiccation.

The liquor from which the ammonia had separated the alumine already spoken of still gave slight precipitates by the carbonat of potash and the oxalat of ammonia; which proves that it contained a little lime.

That portion of the matter not dissolved by the potash weighed at most 0.1 gramme, and appeared to me to be *silex*. Mellite then contains a small quantity of lime and *silex*.

After ascertaining the kind of matter which composed my residuum, I then examined the liquor, which I concluded

must contain acid of mellite united to potash; and in the hope that it would give up its base to mineral acids, I put into a portion of the liquor a few drops of nitric acid, which produced a very slight effervescence, and gave birth to a small quantity of a brown flaky matter. Some hours after what I suspected took place; the acid of mellite crystallised under the form of small short prisms with brilliant facets.

Finding that this method might enable me to separate the above acid from the potash, I exposed the whole of the liquor to a gentle heat, and mixed with it some nitric acid till it had an excess sensible to the taste. I then made it pass through the filter, in order to separate the brown flaky matter, and to obtain the acid in a purer state. In two crystallisations I indeed obtained about 1.34 grammes of it, which were pretty white, though it had still a yellowish tint. The properties which it exhibited by its mixture with other substances were as follow :

1st. This acid has brilliant facets, a considerable degree of hardness, and a slight acid flavour accompanied with a little bitterness, which may have arisen from some particles of bitumen that remained attached to it, and which gave it a yellowish colour.

2d. A portion of this acid, when exposed to the flame of the blow-pipe, exhibited at first some scintillations like salt-petre; it then swelled up, and left a matter which soon penetrated the charcoal.

3d. When heated in a covered platina crucible, it at first swelled up, then became carbonaceous without producing an oily smoke, and left a light charcoal which was exceedingly alkaline \*. This acid remained therefore united to a certain quantity of potash, notwithstanding the excess of nitric acid added to its solution. The same effect took place also with the tartareous and oxalic acids, which by these means pass to the state of *acidulous* salts.

4th. This salt is very little soluble, but I have not been

\* This acid, on account of the above property, cannot be confounded with the acidulous tartrate of potash; for the latter swells up much more, and during its decomposition emits a pretty thick smoke, which has a peculiar odour easily distinguished.



able to determine exactly the proportion of water it requires.

5th. Some grammes of the same acid dissolved in water, being mixed, 1st, with a solution of lime, immediately formed a white flaky precipitate, which soon deposited itself at the bottom of the liquor: 2d, with a solution of the sulphat of lime, a light granulated and crystallised precipitate, which left to the water a little transparency, but which was increased and rendered flaky by the addition of a drop of ammonia\*: 3d, with a solution of the muriat of barytes, a very small precipitate at first, but some moments after a multitude of crystals in the form of needles: 4th, with a solution of silver, a white precipitate, silky and brilliant like a solution of soap; some time after it deposited itself under the form of dust: 5th, with a solution of lead by nitric acid, a white pulverulent and very heavy precipitate: 6th, with a solution of mercury, a white precipitate which was rendered black by a drop of ammonia.

From the result of these experiments it appears the acid of the mellite has a great many properties analogous to those of the acid of sorrel; and by the comparison which I made, I could perceive no other differences than the following: 1st, The precipitate which it occasions in the solution of sulphat of lime manifests itself less speedily, and is crystalline, instead of being pulverulent like that formed by the acidulous oxalat of potash. 2d, It seems less acid to the taste than the acidulous oxalat of potash, but this may be owing to my not having added to its combination with potash enough of nitric acid to deprive it of a sufficient quantity of that alkali. 3d, It swells up a little more by heat than the acidulous oxalat of potash.

In a word, the sublimated salt, the large quantity of car-

\* The acidulous tartrate of potash does not immediately produce a precipitate in a solution of the sulphat of lime, but in 24 hours after there are formed in the mixture crystals with very brilliant facets, which are a compound of lime and tartareous acid. Though crystallised, this tartrate of lime has no resemblance to that produced by the acid of honey-stone with the same matter; it differs from it by swelling up in the fire, whereas the other is decomposed without swelling up; and in this has an analogy to the oxalat of lime.

bonic acid, that of water, and the small portion of charcoal which the mellite furnishes by distillation, are all facts which seem to concur to prove the identity of these two acids; for the salt of sorrel exhibits in the fire the same phenomena\*.

The octaedral form of the mellite seems also to have an analogy with that of the oxalic acid, which is a rectangular prism terminated by pyramids of four faces: to be certain of this, nothing is necessary but to compare the inclination of the faces.

However, as I had at my disposal only about 1.34 gramme of this acid, I was not able to subject it to all the tests necessary to demonstrate, in a positive manner, its identity in every point with the oxalic acid; for, though they exhibited analogous phenomena in all the comparative experiments I made, it is possible that by others which may be made hereafter there may occur one difference sufficient to destroy the resemblance.

I have therefore published this notice chiefly with a view of inducing the chemists of Germany, where this substance is most commonly found, to repeat the analysis of it, and to compare, under every point of view, its acid with that of sorrel. Should my opinion be confirmed by new experiments, we shall then have oxalic acid in the three kingdoms of nature, *viz.* in the state of acidulous oxalat of potash in several kinds of vegetables; in that of oxalat of lime in human urinary calculi; and, in the last place, in the state of oxalat of alumine in the interior of the earth among bituminous kinds of wood; but, in whatever place found, it seems always to be indebted for its origin to vegetable matters†.

X. Dr.

\* The acid of sorrel, or oxalic acid, is that which furnishes by distillation the largest quantity of carbonic acid and water; because, of all the natural vegetable acids known, it contains most oxygen.

† Since the above paper was written, I conceived the idea of mixing the acid of honey-stone, united to a little potash, with a solution of pure sulphat of alumine, and there was immediately formed a very abundant flaky deposit: on the other hand, I put into a solution of the same salt acidulous oxalat of potash, but there were no signs of precipitation.

These different effects, therefore, ought to excite well-founded doubts respecting the identity of the acid of honey-stone and the oxalic acid; and

I confess



X. Dr. DICKSON'S Translation of CARNOT on the  
*Infiniteſimal Calculus.*

[Continued from Page 240.]

*The fundamental Principles of the Infiniteſimal Analyſis.*

32. **THEOREM I.**—*If in any imperfect equation whatever, there be ſubſtituted for any one of its component quantities, another quantity differing infinitely little from it, or whoſe ratio to the firſt hath unity for its limit, or ultimate value; then, I ſay, that the equation reſulting from this transformation cannot be a false equation, that is, it will become abſolutely exact, or, at leaſt, will remain what I call an imperfect equation.*

For ſince, by the hypotheſis, there has been ſubſtituted for one quantity another of the ſame ultimate value, and whoſe ratio to the former hath unity for its limit, it is evident that ſuch ſubſtitution could not change either the ultimate values of the ſides of the propoſed equation, or their ultimate ratio. Now, by the hypotheſis, this ultimate ratio was (1) unity before the ſubſtitution; therefore it will ſtill remain unity; and conſequently the equation will preſerve the character of what I call an imperfect one, if it do not become rigorouſly exact. Q. E. D.

33. **THEOREM II.**—*An equation which contains only aſſigned quantities, cannot be an imperfect equation.*

For, by the definition of imperfect equations (article 31), their ſides are unequal, but differ infinitely little from each other, their ratio approaching, as nearly as we pleaſe, to the ratio of equality; therefore there enters into ſuch an equation ſome quantity which makes no part of the ſyſtem of the quantities propoſed. But by the hypotheſis the propoſed

I confeſs that I ſhall ſuſpend my judgment on this point, notwithſtanding the inclination which I firſt had to believe theſe two acids to be of the ſame nature. For this reaſon, before any thing can be determined, we muſt wait until experiments made on a larger ſcale throw more light on the ſubject.

equation contains assigned quantities only, and, consequently, it cannot be what I call an imperfect equation. Q. E. D.

34. THEOREM III.—*Every imperfect equation which hath undergone such transformations as are indicated in the first theorem, and from which all unassigned quantities have been eliminated, by those transformations, will be necessarily and rigorously exact.*

For, by the first theorem, the equation cannot be absolutely false; and, by the second, it cannot be imperfect; therefore it is necessarily and rigorously exact. Q. E. D.\*

35. COROLLARY.—All that hath been said on the subject of imperfect equations, ought to be understood of all the proportions, propositions, and reasonings whatsoever, which can be expressed and delivered by such equations.

*The leading Principle of this Analysis.*

36. SCHOLIUM.—Such are the general principles into which the theory of the Infinitesimal Calculus is resolvable. From these principles it appears, that if, after expressing the conditions of a problem in imperfect equations, we arrive, by means of such transformations as are indicated in the first theorem, at the elimination of all auxiliary or unassigned quantities, a compensation of errors must necessarily have taken place, in the course of the process. It further appears, that the advantage of the Infinitesimal Calculus consists in this, That, the conditions of a question being often very difficult to be expressed accurately by rigorous equations, it is easy to do it by imperfect equations, from which as certain results can be derived, as if the original equations had been perfectly accurate; and this by the simple expedient of eliminating the quantities whose presence occasioned the errors.

\* The word *false*, as before intimated (in the Note on § 9), seems by far too strong a term to be applied, in any allowable sense, to such equations as the author is considering. He should have defined it, and adhered to his definition; for, in art. 31, he uses *false* and *imperfect* as convertible terms, and, in art. 32 and 33, he takes them in opposite senses. This unsteadiness renders his meaning somewhat ambiguous. But if, by *false* or *imperfect* equations, he uniformly mean such whose sides differ infinitely little from equality, then his whole meaning becomes clear, and his three theorems almost self-evident.—W. D.



The reason of this procedure is simple. Suppose we have occasion to investigate the relations which subsist between several proposed quantities. If it be difficult to find directly equations to express these relations, we naturally recur to some intermediate quantities, which may serve as terms of comparison. By this means we obtain, if not the very equations sought, at least other equations, in which the proposed quantities are blended with auxiliary ones; and there can be no question that these last ought to be eliminated. But if, additionally, the values of these auxiliary quantities be arbitrary, and may be supposed as small as we please, without affecting the proposed quantities, it is easy to see, that, if in the equations expressing the relations sought, arbitrary quantities be mixed with proposed ones, each of these equations may be decomposed into two, one containing assigned, and the other arbitrary quantities. It is nearly in this manner, that an equation containing real and imaginary quantities may be decomposed into two equations, the one consisting of real, and the other of imaginary quantities. Now, as we only want the equation which exists between the proposed quantities, it is evident that, in those equations where they are mixed with arbitrary ones, we may safely neglect the quantities which embarrass our calculation, when the resulting errors can only affect the equation between the arbitrary quantities which it contains. Now this is precisely what takes place in the Infinitesimal Calculus, where we consider infinitely small quantities as nullities when compared with finite ones.

In order to render this explanation still clearer, let us resume our former example. In article 9, we found

$$TP + T'T = y \frac{MZ}{RZ}, \text{ and } \frac{MZ}{RZ} = \frac{2y + RZ}{2a - 2x - MZ}.$$

These two equations are both perfectly exact, whatever be the values of  $MZ$  and  $RZ$ . Deducing, then, from the first equation, the value of  $\frac{MZ}{RZ}$ , and substituting it in the second, I get

$$\frac{TP + T'T}{y} = \frac{2y + RZ}{2a - 2x - MZ};$$

and this equation is accurate, as it ought to be, whatever distance

distance we may suppose to intervene betwixt the lines  $RS$  and  $MP$ .

Now, it is easy to see that this last equation is susceptible of the following form :

$$\left( \frac{TP}{y} - \frac{y}{a-x} \right) + \left( \frac{T'T}{y} - \frac{yMZ + aRZ - xRZ}{(a-x) \cdot (2a-2x-MZ)} \right) = 0^*,$$

an

\* As this equation appears to be erroneous, I shall examine it throughout ; putting, for brevity,  $TP = s$ ,  $T'T = \dot{s}$ ,  $MZ = \dot{x}$ ,  $RZ = \dot{y}$ . The equation immediately preceding, in this notation, is

$$\frac{s + \dot{s}}{y} = \frac{2y + \dot{y}}{2a - 2x - \dot{x}}, \text{ or } \frac{s}{y} + \frac{\dot{s}}{y} = \frac{2y + \dot{y}}{2a - 2x - \dot{x}},$$

which being made  $= 0$ , is  $\left( \frac{s}{y} + \frac{\dot{s}}{y} \right) - \left( \frac{2y + \dot{y}}{2a - 2x - \dot{x}} \right) = 0$ .

Now, (by article 9)  $s = \frac{y^2}{a-x}$ , or  $\frac{s}{y} = \frac{y}{a-x}$ , or  $\frac{s}{y} - \frac{y}{a-x} = 0$ ;

and this will be the first member of the new equation, which is to contain what our author calls "assigned" quantities only, as the second member is to contain none but "auxiliary" quantities. The first part of the

second member is obviously  $\frac{\dot{s}}{y}$ , an auxiliary quantity ; and, in order

to render the second part,  $-\left( \frac{2y + \dot{y}}{2a - 2x - \dot{x}} \right)$  also wholly auxiliary, we multiply both the numerator and denominator by  $a-x$  (or by its equal  $\frac{y\dot{y}}{\dot{x}}$ , by art. 9). This multiplication by  $a-x$ , gives the latter part of the numerator  $ay - xy$  ; and thus far the author's new equation is right.

But the auxiliary, equivalent to the assigned quantity  $2y \cdot (a-x)$ , cannot be what he makes it ; for, as we have just seen,  $a-x =$

$$\frac{y\dot{y}}{\dot{x}}, \text{ and therefore it should be } 2y \cdot (a-x) = \frac{2y^2\dot{y}}{\dot{x}}.$$

Now the author has  $2y \cdot (a-x) = y\dot{x}$ , and consequently, if his result be right, we should have,

$$\frac{2y^2\dot{y}}{\dot{x}} = y\dot{x}, \text{ and } y = \frac{\dot{x}^2}{2\dot{y}}.$$

But (by art. 9, again)  $\frac{y}{a-x} = \frac{\dot{x}}{\dot{y}}$ , and  $y = \frac{a\dot{x} - x\dot{x}}{\dot{y}}$ ;

$$\text{Therefore } \frac{a\dot{x} - x\dot{x}}{\dot{y}} = \frac{\dot{x}^2}{2\dot{y}}, \text{ and } a-x = \frac{\dot{x}}{2};$$

But this being plainly impossible, I conclude that  $2y \cdot (a-x) = \frac{2y^2\dot{y}}{\dot{x}}$ , or  $2y^2\dot{y} : \dot{x} (= 2y^2 : (RZ : MZ) \text{ in our author's notation})$

and



an equation, the first term of which contains only given quantities, and the second only arbitrary quantities, which and that the new equation should be,

$$\left(\frac{s}{y} - \frac{y}{a-x}\right) + \left(\frac{\dot{s}}{y} - \frac{2y^2\dot{y} : \dot{x} + a\dot{y} - x\dot{y}}{(a-x) \cdot (2a-2x-\dot{x})}\right) = 0,$$

$$\text{or} \left(\frac{TP}{y} - \frac{y}{a-x}\right) + \left(\frac{T'T}{y} - \frac{2y^2 \cdot (RZ : MZ) + aRZ - xRZ}{(a-x) \cdot (2a-2x-MZ)}\right) = 0;$$

and, in this last manner, I shall, in future, take the liberty to write it.

If the value of  $a-x$  had been taken from the accurate equation (in article 9)  $\frac{\dot{x}}{\dot{y}} = \frac{2y + \dot{y}}{2a - 2x - \dot{x}}$ , we should then have had

$$2y \cdot (a-x) = (2y^2\dot{y} + y\dot{y}^2 + y\dot{x}^2) : \dot{x};$$

so that, in rigid strictness, the new equation should be,

$$\left(\frac{s}{y} - \frac{y}{a-x}\right) + \left(\frac{\dot{s}}{y} - \frac{(2y^2\dot{y} + y\dot{y}^2 + y\dot{x}^2) : \dot{x} + a\dot{y} - x\dot{y}}{(a-x) \cdot (2a-2x-\dot{x})}\right) = 0.$$

Now, though  $y^2$  and  $x^2$  be infinitely less *still* than their infinitely small roots  $y$  and  $x$ , and consequently  $y\dot{y}^2 + y\dot{x}^2$  infinitely less than  $2y^2\dot{y}$ , and thus may be safely neglected; yet it would not, perhaps, have been amiss, if the author, while he was exhibiting all his quantities, had brought *them* also into view.

Some readers may be surprised at the mention of quantities infinitely less than infinitely small ones. But their wonder will cease when they recollect, That if any integer, or any fraction, be multiplied by a fraction, the product will be *less* than the multiplicand, and will, in fact, be only such a part of the multiplicand, as the multiplier is of unity. Thus, if .ooooooooooooo1 (prefixing cyphers *ad infinitum*) be multiplied by any other interminably or inconceivably small fraction, the product will be only such an inconceivably small part of the *already* inconceivably small multiplicand, as is expressed by the inconceivably small multiplier: in other words, the product (relatively to our conceptions) may be said to be infinitely less than the infinitely small multiplicand. In like manner,  $\dot{x}y$  may represent a rectangle, whose breadth  $\dot{x}$  is infinitely small compared with its length  $y$ , which may be any finite line, or it may even be an indefinitely or infinitely great line. But now suppose  $y$  also to be infinitely small, or to become  $\dot{y}$ ; then it is easy to see, that this second rectangle  $\dot{x}\dot{y}$ , both whose dimensions are infinitely small, will be infinitely less than the first rectangle  $\dot{x}y$ , which has only one of its dimensions infinitely small. Thus also the squares  $\dot{x}^2$  and  $\dot{y}^2$  will be infinitely less than their infinitely small roots  $\dot{x}$  and  $\dot{y}$ , and the cubes  $\dot{x}^3$  and  $\dot{y}^3$ , than the squares  $\dot{x}^2$  and  $\dot{y}^2$ , &c.—*Neque enim novit natura limitem.*

Were this the proper place, we might recommend these and many analogous considerations, both mathematical and metaphysical, to the serious attention of certain gentlemen, who, without abating a tittle from their high pretensions to accurate reasoning, scruple not to tell us, in very general terms, "that they cannot believe any thing which they cannot conceive or comprehend."—W. D.

last may be supposed as small as we please, without affecting the quantities in the first term; because we may suppose  $RS$  to be as near as we please to  $MP$ . Agreeably, therefore, to the theory of indeterminate quantities, each of the terms of this equation, taken separately, must destroy itself, or be equal to nothing; that is, this equation may be resolved or decomposed into two others, namely,

$$\frac{TP}{y} - \frac{y}{a-x} = 0, \text{ and } \frac{T'T}{y} - \frac{2y^2 \cdot (RZ : MZ) + aRZ - xRZ}{(a-x) \cdot (2a - 2x - MZ)} = 0,$$

the first of which equations contains only assigned quantities, and the second none but arbitrary ones. But assigned quantities alone are necessary to our purpose; for they give us the required value of  $TP$ , the same that we before found it. When, therefore, we have even committed errors in the course of the calculation, the exactness of the result will not be affected, provided that those errors are confined to the second equation. And this, in fact, is the same thing which would have happened if we had considered  $MZ$ ,  $RZ$ , and  $T'T$ , as nullities, in comparison with the quantities  $a$ ,  $x$ , and  $y$ . We should, indeed, have committed errors in expressing the conditions of the problem; but these errors would have destroyed each other by compensation, and the required result would not, in any respect, have been altered.

*The Infinitesimal Analysis is only an Application or Extension of the Method of proceeding in Indeterminate Problems.*

37. From what has been said, it will be easy to perceive that the Infinitesimal Analysis is nothing else than an application, or, if you will, an extension of the Method of Indeterminates. For, agreeably to that method, I say, that when we neglect an infinitely small quantity, we do nothing more, properly speaking, than *understand* it, and do not suppose it to be nothing. Thus, when instead of the two exact equations, found in article 9, namely,

$$TP + T'T = MP \frac{MZ}{RZ}, \text{ and } \frac{MZ}{RZ} = \frac{2y + RZ}{2a - 2x - MZ},$$

I employ the two imperfect equations,

$$TP = MP \frac{MZ}{RZ}, \text{ and } \frac{MZ}{RZ} = \frac{y}{a-x};$$

I know



I know very well that I am committing an error, and I put the equations, *mentally*, so to speak, into this form,

$$MP \frac{MZ}{RZ} = TP + \phi, \text{ and } \frac{MZ}{RZ} = \frac{y}{a-x} + \phi';$$

$\phi$  and  $\phi'$  being such quantities as the former equations want to render them exact. In like manner, in the equation

$$\frac{TP}{MP} = \frac{y}{a-x},$$

resulting from the above two imperfect equations, I *understand* the quantity  $\phi''$ , being such that  $\left( \frac{TP}{MP} - \frac{y}{a-x} \right) + \phi'' = 0$ , may be an exact equation. But I

know well enough, that this last quantity  $\phi''$  is equal to zero; or, at least, that it is only an infinitely small quantity, since no-infinitesimal enters into the first term. Now this cannot happen, unless each of the terms, taken separately, be equal to nothing; whence I conclude, that I have exactly

$$\frac{TP}{MP} = \frac{y}{a-x};$$

so that the quantities  $\phi$ ,  $\phi'$  and  $\phi''$  have not been suppressed as nullities, but only *understood*, in order to simplify the calculation.

Again: if  $X$ , for example, be an arbitrary quantity, which may be rendered as small as we please, and if there were given an equation of this form,

$$A + BX + CX^2 + \&c. = 0,$$

$A$ ,  $B$ ,  $C$ , &c. being independent on  $X$ , this equation cannot exist, unless it be  $A = 0$ ,  $B = 0$ ,  $C = 0$ , &c.; that is, unless each term, taken separately, whatever be their number, be equal to zero. And, for the same reason, if we have an equation of this general form,  $P + Q = 0$ ; so that  $P$  may be a function of the quantities given or determined by the conditions of the problem; and, on the other hand,  $Q$ , a quantity which we may suppose as small as we please, we shall necessarily have  $P = 0$ , and  $Q = 0$ . But such is precisely the nature of the equation in the last article, namely,

$$\left( \frac{TP}{y} - \frac{y}{a-x} \right) + \left( \frac{T'T}{y} - \frac{2y^2 \cdot (RZ : MZ) + aRZ - xRZ}{(a-x) \cdot (2a - 2x - MZ)} \right) = 0.$$

Therefore each of the terms of this equation, taken separately, is equal to zero; and consequently, the quantities  $T'T$ ,  $MZ$  and  $RZ$ , which enter not into the first term, may

be neglected, in the course of the calculation, without altering that first term.

The Infinitesimal Analysis, therefore, differs from the method of indeterminates only in this, that in the former, quantities which, were they allowed to remain, would, in the end, always destroy one another, are treated as nothing, or rather are *understood* throughout the calculation; while, in the Method of Indeterminates, we wait till the end of the calculation, and then cancel the arbitrary quantities which ought to be eliminated. This last method may therefore very easily be made to supply the use of the Infinitesimal Calculus, without the help of imperfect equations, and without committing any error in the course of the calculation.

38. There is yet another method of coming at the results of the Infinitesimal Analysis, without overpassing the bounds of ordinary algebra; and that is, by the Method of Limits, or Ultimate Ratios. For though this analysis be founded entirely on the properties of limits and ultimate ratios, it differs nevertheless from what is properly called the method of limits, in this, that in the latter, the quantities which we call Infinitesimal, do not enter separately into the calculation, nor even their ratios, but only the ultimate values of these ratios, which being finite quantities, do not so properly constitute this method a particular calculus, as a simple application of ordinary algebra.

The business before us, then, is by barely introducing into ordinary algebra, not Infinitesimal quantities themselves, but the ultimate ratios of these quantities, to supply the means which the Infinitesimal Analysis furnishes, for discovering any properties, ratios and relations whatsoever, of the magnitudes which constitute any proposed system; and this is that which is properly called the Method of Limits.

To explain the procedure, and give some idea of the spirit, of this method, we shall again resume the example before treated of.

*Explanation of the Method of Limits, properly so called.*

It is evident, from what was delivered in article 9, that, though  $\frac{MZ}{RZ}$  be not equal to  $\frac{TP}{MP}$ , yet the first of these quantities



quantities differs so much the less from the second, as  $RS$  approaches nearer to  $MP$ ; or, in other words, that  $\frac{MZ}{RZ} = \frac{TP}{MP}$  is an imperfect equation; but that (putting  $L$  for the limit, or the ultimate value,)  $L \cdot \frac{MZ}{RZ} = \frac{TP}{MP}$  is a perfect, or rigorously exact, equation.

In like manner,  $L \cdot \frac{MZ}{RZ} = \frac{y}{a-x}$  is proved to be a perfect, or rigorously exact, equation. Equating then these two values of  $L \cdot \frac{MZ}{RZ}$ , there arises, as before,

$$\frac{TP}{MP} = \frac{y}{a-x}, \text{ or } (MP \text{ being } = y) \quad TP = \frac{y^2}{a-x}.$$

Thus, this new calculus contains neither the infinitely small quantities  $MZ$  and  $RZ$ , nor even their ratio  $\frac{MZ}{RZ}$ ; but only the limit or ultimate value of that ratio, namely,  $L \cdot \frac{MZ}{RZ}$ , which is a finite quantity.

39. If this method could be always as easily put in practice as the ordinary Infinitesimal Analysis, it might even appear the most eligible of the two: for it would have the advantage of conducting us to the same results, by a path which is always direct and luminous; whereas the other conducts us to the truth, only after having made us traverse, so to speak, the regions of error.

But it must be owned that the Method of Limits is attended with a considerable difficulty, which has no place in the ordinary Infinitesimal Calculus. In the former, the infinitely small quantities cannot, as in the latter, be separated from each other; and these quantities being always connected two and two, afford no opportunity of introducing into the combinations, the properties of each in particular, or of subjecting the equations into which they enter, to those transformations which may assist in their elimination. This difficulty is much less felt in the operations themselves, than in the preparatory and supplemental propositions and reasonings.

*The Origin of the Name, Infinitely Small Quantities.*

40. From what has been said (in article 2.) on the origin which the Infinitesimal Analysis might have had, it appears, that the quantities called Infinitely Small, received that name from it's being at first believed that it was necessary, for the success of the calculations in which they were employed, to attribute to those arbitrary quantities values, which were really less than any which could be recognized by the senses, or conceived by the imagination. But a better digested theory has made it appear, that such a supposition is unnecessary; since the success of this calculus proceeds not from the attenuation of those arbitrary quantities, but solely from the compensation of errors which they occasion in the process.

We have seen indeed, in illustrating the example so often adduced, that the procedure and the results of the calculation were precisely the same, whatever value we attributed to the infinitely small quantities  $MZ$  and  $RZ$ , and that consequently the character of this kind of quantities consists not in their real minuteness, but rather in their being absolutely indeterminate, that is, in their property of remaining arbitrary throughout the calculation, and so independant on the proposed quantities, that we can always take them as small as we please, without changing, in any respect, the conditions of the problem.

Infinitesimal quantities, as was observed in article 24, are by no means chimerical beings, but simply variable quantities, characterized by the nature of their limit, which is 0 for infinitely small quantities, and  $\frac{1}{0}$  for those which are infinitely great. To these indeterminate quantities, as well as to all other indefinite quantities, may be successively attributed, divers arbitrary values, and among those values ought to be included the ultimate values of all, that is, 0, for quantities infinitely small, and  $\frac{1}{0}$  for those which are infinitely great.

*Distinction*



*Distinction of Mathematical Infinity into Sensible and Absolute.*

41. This observation leads to the distinction of mathematical infinity into two kinds, namely, *sensible*, or *assignable*, infinity, and *absolute*, or *metaphysical*, infinity, which is the limit of the former.

If then, to any infinitely small quantity, be assigned a determinate value, which is not 0, this value will be what I call a *sensible*, or *assignable*, Infinitesimal; whereas, if this value be the last of all, that is, if it be absolutely nothing, it will be what I call an *absolute*, or *metaphysical*, Infinitesimal, which I shall also distinguish by the name of an *evanescent* quantity. Thus an evanescent quantity is not that which is generally called an infinitely small quantity, but only the ultimate value of that quantity. It is only, I say, a determinate value which, like any other value, may be attributed to that arbitrary quantity, which is generally denominated infinitely small.

42. The consideration of these evanescent quantities would be almost useless, if in calculation we were restricted to treat them as simple nullities; for, in that case, they would present only the vague ratio of 0 to 0, which is no more equal to 2 than it is to 3, or to any other quantity whatsoever. But it must not be forgotten, that these nullities are here invested with particular properties, as the ultimate value of indefinitely small quantities, whose limits they are; and that the particular epithet, *evanescent*, is applied to them in order to denote, that, of all the ratios and relations of which they are susceptible in quality of nullities, no other is considered as entering into the calculation, than those which the law of continuity assigns to them, when the system of auxiliary quantities is supposed insensibly to approach to the system of assigned quantities. This idea is what some great geometers have thought they could express, when they said, that evanescent quantities were quantities considered, neither before nor after they had vanished, but in the very instant of their vanishing\*.

For

\* (*C'est ce que de grands geometres ont cru pouvoir exprimer, &c.*)  
The author here plainly alludes to Sir I. Newton, the author of this doctrine

For example, in the case before adduced, as long as  $RS$  does not coincide with  $MP$ , the fraction  $\frac{MZ}{RZ}$  is greater than  $\frac{TP}{y}$ ; nor do these fractions become equal, till  $MZ$  and  $RZ$  are reduced to nothing. It is true, that *then*,  $\frac{MZ}{RZ}$  is as much equal to any other quantity as to  $\frac{TP}{y}$ ; because  $\frac{0}{0}$  is a quantity altogether arbitrary; but, among all the different values which  $\frac{MZ}{RZ}$  may be supposed to have,  $\frac{TP}{y}$  is the only one which is subjected to the law of continuity and determined by it. For, if a curve were constructed, whose abscisse

doctrines of prime and ultimate ratios, and of the whole method of Fluxions. That great man, in the concluding scholium of Sect. 1. B. 1. of the *Principia*, has these words: "*Objeçtio est, &c.*" "It may be objected that evanescent quantities have no ultimate proportion; for that, before they vanish, that proportion cannot be the last, and after they have vanished, it is nothing. But, by the same argument, when a moving body stops at a certain place, it may be said that it has no ultimate velocity, for that, before the body reaches that place, the velocity is not the ultimate velocity, and when it has reached the place, the velocity is nothing. The answer is easy; for by the ultimate velocity is meant the velocity of the body, neither *before* it reaches its last place, nor *after* it has reached it, but that velocity with which it actually reaches it; that is, the very velocity with which the body attains its last place, and comes to rest. In like manner, by the ultimate ratio of evanescent quantities is to be understood, the ratio of those quantities, neither *before*, nor *after* they vanish, but *the ratio with which they vanish*. And thus also the prime ratio of nascent quantities is that ratio with which they first start into existence," &c.

Though but a humble and distant follower of Newton,

*Quem longe sequor, et vestigia pronus adoro,*

I see nothing that could hinder him from "thinking he could express" the fundamental principle of this doctrine by such language. For my own part, I must frankly say, that the scholium whence it is quoted, conveys, or suggests, that principle more clearly to my mind, than all that our ingenious author and others have written on the subject. But we do not all see things, with equal clearness, in the same point of view. Some of my superiors in genius and knowledge have a different opinion of that scholium, and of the rest of Newton's fluxionary doctrine, as delivered by himself, and even as explained by Ditton, Simpson, and others. To such I would recommend the present perspicuous tract.—W. D.

was



was the indefinitely small quantity  $MZ$ , and it's ordinate proportional to  $\frac{MZ}{RZ}$ , that which would answer to the abscisse  $o$ , would be represented by  $\frac{TP}{y}$ , and not by an arbitrary quantity. Now this is what distinguishes the quantities which I call evanescent from those which are simply nothing.

Thus, though in general we have  $o = 2 \times o, = 3 \times o, = 4 \times o, \&c.$  yet we cannot treat an evanescent quantity, such as  $MZ$ , in the same manner, and say  $MZ = 2MZ = 3MZ = 4MZ, \&c.$ ; for the law of continuity cannot assign to  $MZ$  and  $MZ$ , any other ratio than that of equality, nor any other relation than that of identity \*.

43. We have seen that by introducing into the calculation infinitely small quantities, and by neglecting them in comparison with finite quantities, the equations became imperfect, and that the errors which they produced were only compensated in the required result. But we have it now in our power to avoid this kind of inconvenience, by means of evanescent quantities, which being nothing else than the ultimate values of the infinitely small quantities corresponding to them, may, like any of the other values, be attributed to these indefinitely small quantities; and which being, in another point of view, absolute nullities, may be neglected, when they are found connected with any effective quantities, without preventing the calculation from being perfectly rigorous.

44. The Infinitesimal Analysis, then, may be considered in two different points of view; by regarding the infinitely small quantities, either as real, effective quantities, or as absolute nullities. In the first case, the Infinitesimal Analysis is nothing else than the Calculus of compensated errors; and in the second, it is the art of comparing evanescent quantities among themselves, and with others, in order to deduce from these comparisons the proportions and relations, whatever they may be, which subsist among the quantities proposed.

\* For "identity," the author should have used the word "congruity." They are very far from being synonymous terms; though used as such by some mathematicians. See Euclid's 8th axiom.—W. D.

Evanescent quantities, as being equal to nothing, ought to be neglected in the calculation, when they are connected, by addition or subtraction, to any real, effective quantity. But they have, nevertheless, as we have seen, relations very important to be known, relations which are determined by the law of continuity, to which the system of auxiliary quantities is subjected in its mutations. Now, in order to discover this law of continuity, it is easy to perceive, that we are obliged to consider these evanescent quantities at some distance from the limit where they entirely vanish, otherwise they present only the indefinite ratio of 0 to 0; but this distance is arbitrary, and hath no other object but to enable us to judge more easily of the ratios or relations which exist between these evanescent quantities. These are the ratios which we have in view, when we consider infinitely small quantities as absolute nullities, and not those ratios which exist between the quantities which are not yet arrived at their limit, or the term of their annihilation. These last quantities, which I have called indefinitely small, are not themselves designed to enter into the calculus considered in the present point of view; but are only employed to assist the imagination, and to indicate the law of continuity which determines the ratios and relations, whatever they may be, of the corresponding evanescent quantities.

According to this hypothesis, the quantities represented by  $MZ$  and  $RZ$ , in the proportion  $MZ : RZ :: TP : MP$ , are supposed absolutely equal to nothing. But, as it is their ratio that is required, in order to perceive it's equality to  $\frac{TP}{MP}$ , the indefinitely small quantities, which answer to these nullities, must be considered, not that they themselves may be introduced into the calculation, but that the vanishing quantities, which are their ultimate values, may enter into it, under the denominations of  $MZ$  and  $RZ$ .

45. These expressions therefore,  $MZ$  and  $RZ$ , here represent nullities, and are used under the forms of  $MZ$  and  $RZ$ , rather than under the common form 0, because, if they were used under this last form, it would no longer be possible, in the operations wherein they are mixed, to distinguish their different origins, or, in other words, to distinguish the different

ferent



ferent indefinitely small quantities which answer to them. Now the consideration of these last, at least mentally, is necessary to the apprehension of the law of continuity, which determines the required ratio of the evanescent quantities which are their limits; and consequently it is essentially necessary to keep them in view, and to characterize them by expressions which may prevent them from being confounded.

46. Evanescent quantities, which are the subject of the Infinitesimal Calculus, considered in this new point of light, are, it is true, *entia rationis*, creatures of the understanding. But this does not hinder them from having mathematical properties, and from being compared together, as well as imaginary quantities in Algebra \*, which have no better claim to existence. For it is not more certain, for example, that

$$60 \text{ is } = 20 + 40, \text{ than that } \sqrt{-a} \text{ is } = \sqrt{-b} \times \sqrt{\frac{a}{b}}.$$

Now no person doubts the accuracy of the results obtained by the calculus of imaginary quantities; though they be only the algebraic forms and emblems of absurd quantities. With much greater reason are we prevented from rejecting evanescent quantities, which are at least the limits of real quantities, and are *in intimate contact*, so to speak, with their existence. What signifies it, indeed, Whether these evanescent quantities are, or are not, chimerical entities, if their *ratios* be not such, and if *these ratios alone interest us*? When, therefore, we subject infinitesimal quantities to calculation,

\* The author might have added points, lines, surfaces and solids in geometry; for they too are *entia rationis*, which have no existence in external nature. Points are merely the terms or limits of lines, lines of surfaces, and surfaces of solids; just as the prime ratios of nascent, and the ultimate ratios of evanescent, quantities, are the limits of those ratios, when the quantities are considered as beginning, or ceasing, to be. Yet geometry rests on this foundation of abstract entities, with perfect security; for the truth is, that, without abstract ideas, science, *strictly* so called, cannot exist. And he who can conceive a mathematical point, a mere abstract *locus*, a something without parts, an entity truly one and indivisible, which, being a creature of the intellect, entirely eludes the cognizance of every sense; I say such a man will no sooner understand Newton's doctrine of prime and ultimate ratios, than he will esteem it a legitimate foundation of mathematical reasoning.—W. D.

we have it entirely in our power to consider them either as real quantities, or as absolute nullities. The difference between these two ways of considering this question, consists in this, that, by regarding evanescent quantities as nullities, the propositions, equations and results, whatever they may be, are always accurate and rigorous; but have a reference to quantities which are creatures of the understanding, and express the relations which exist between quantities which do not themselves exist\*. On the other hand, by considering infinitely small quantities as having some reality, the propositions, equations and results, whatever they may be, have for their subject real quantities. But these last propositions, equations and results are false, or rather imperfect, and become exact in the end only in consequence of the compensation of their errors, a compensation, however, which is the necessary and infallible result of the operations of the calculus.

\* Thus the ratios of the ordinal numbers (*one, two, three, &c.*) to each other, while those numbers remain floating, so to speak, in indeterminate abstraction, and unapplied to any particular objects of sense, may be said to “express the relations which exist between quantities which do not themselves exist.” Thus also, if a body be *supposed* to fall from any moderate height, its velocities at any two points (resistance apart) will have to each other the ratio of the square roots of the spaces supposed to be described; although no body ever *actually* fell, or perhaps ever will actually fall, from that precise height. These examples, it is hoped, will prevent readers who are not much accustomed to such speculations, from rashly charging our author with absurdity, in talking of the relations between quantities which do not themselves exist; that is, which have no existence in external nature. For the truth is, and a surprising, unaccountable truth it appears to many beginners, that the objects of Pure Mathematics, though originally abstracted, or copied, from external objects, have no existence out of the minds which conceive them; and hence proceeds all that accuracy for which those sciences are justly valued. The inaccuracy of the figures, motions, &c. of external objects induces a corresponding inaccuracy into Mixed Mathematics.—The inaccuracy of language has an analogous effect in metaphysics; for metaphysical relations and deductions may be perfectly accurate *in the mind*, yet few of them can be adequately and unexceptionably expressed, for want of an accurate, unambiguous language. Hence the endless disputes with which men unhappily disposed to cavil, and who affect to doubt of every thing, never cease to embroil that important, and otherwise not unpleasant, region of philosophy.—W. D.



47. The theory which has been thus expounded easily furnishes answers to all the objections which have been made against the Infinitesimal Analysis, the principle of which several geometers have thought faulty, and capable of leading to erroneous conclusions. But those gentlemen have been overwhelmed, if the expression may be allowed, with a multitude of prodigies, and with the splendour of the numerous truths wherewith this principle has teemed.

These objections may be reduced to this. The quantities said to be infinitely small, are either absolute nullities, or they are not; for it is ridiculous to suppose that there exist entities, which hold a middle place between quantity and nullity. Now if they are absolute nullities their comparison leads to nothing, for the ratio of  $o$  to  $o$  is no more  $a$  than it is  $b$ , or any other quantity whatever. If, on the other hand, they are not nullities, but real quantities, they cannot without error be neglected, as the rules of the Infinitesimal Analysis prescribe.

The answer is simple. So far from its not being logical to consider infinitely small quantities, either as real beings, or as nothings, they may, on the contrary, be treated at pleasure, either as nullities or as true quantities. For they who wish to consider them as nullities, may answer, that what they call infinitely small quantities are not any nullities taken at random, but nullities assigned by the law of continuity which determines their relation\*; that among all the relations of which these quantities are susceptible as nullities, they only consider those which are determined by this law of continuity; and, in a word, that these relations are not vague and arbitrary, because the law of continuity does not assign several different relations between the differentials, for example, of the abscissa and ordinate of a curve, when these differentials vanish, but one only, which is that of the subtangent to the ordinate.

On the other hand, they who regard infinitely small quantities as true ones, may answer, that what they call an infinitely small quantity is one which is arbitrary and independant on the qualities proposed; that, therefore, without supposing

\* See the Note on article 22.

it nothing, it may be treated as such, and yet no error will exist in the result; because that error, if there were any, would be arbitrary, like the quantity which occasioned it. Now it is evident, that no such error can exist, except among quantities, one of which at least is arbitrary. When, therefore, we arrive at a result containing no arbitrary quantity, and which expresses any relation whatever between quantities given, and those determined by the conditions of the problem, we may rest assured that that result is accurate; and that, consequently, the errors necessarily committed in expressing these conditions, must have been compensated and have disappeared, by the necessary and infallible effects of the operation.

48. Other mathematicians, apparently embarrassed by the objection just discussed, have simply confined themselves to prove, that the Method of Limits, the processes of which are rigorously accurate, in all respects, must necessarily lead to the same results as the Infinitesimal Calculus. But, while it is agreed that the principle of that method is very luminous, it cannot be dissembled, that the difficulty is thus only eluded, not removed; that the Method of Limits leads to the same results as the Infinitesimal Calculus, only by a difficult and circuitous way; and, in fine, that that method, far from being the same with the Infinitesimal Calculus, is, on the contrary, only the art of dispensing with this calculus, and of supplying it by ordinary Algebra. It appears to me, that they would succeed, in a more simple manner, by the Method of Indeterminates. But why adopt one of these methods to the exclusion of the rest, when they can afford us their mutual assistance? Let us then employ them all—the Infinitesimal Calculus, properly so called, the Method of Limits, and the Method of Indeterminates, as circumstances may require, and let us neglect none of the means which can conduct us to truth, or simplify our researches.

It remains for me to show, by some examples, the application of the general principles, which I have explained. This I shall do, by giving my reader an *idea* of the *Differential* and *Integral Calculi*, which, properly speaking, are the Infinitesimal Analysis itself reduced to practice.

[To be continued.]



XI. *Account of C. F. DAMBERGER'S Travels through the interior Parts of Africa, from the Cape of Good Hope to Morocco.*

[Continued from p. 253.]

ON the 15th of December our traveller proceeded on his journey, and after passing through three villages rested for some time near a fourth under a beautiful *matabora* tree\*. Here he was visited by some of the natives, who offered him a piece of wolf's flesh, which he at first refused; but finding he could get nothing else, he ate it, and thought it tasted better than that of the buffalo. Two miles further he found in a wood a prodigious number of tortoises, which exciting his appetite, he resolved to spend the night there under some trees. He therefore made a large fire, and roasted some of the flesh of these animals, but ate so freely, that for some time he could not sleep. Towards morning he fell asleep, but had scarcely remained an hour in that state, when he found something move at his feet; upon which he started up, and discovered a snake three ells in length and a foot in thickness, devouring the remains of his meal. Being much alarmed at the appearance of this guest, he immediately hastened from the spot and continued his journey. Towards evening he arrived at a narrow but long ridge of mountains, where he rested for the night.

Next morning, when about to depart, he heard human voices, and soon after saw about twenty armed men, who were escorting a company of prisoners, coupled together, two and two, with thongs. Some of the former immediately sprung towards him, and conducted him to their leader, who, after surveying him for some time, took a thong which was wound round his middle, and, having bound with it his hands and feet, ordered him to follow him. Towards noon they arrived at the large village of Cuojaha, where they rested, and procured a supply of water. This was the first village in the

\* The leaves of this tree are small and long; the branches hang down, and the flowers are of a reddish colour. The fruit are round, and about the size of a large pea; they contain a kind of meal of an excellent taste, which is formed into a sort of cakes not inferior to biscuit.

province of Porguhomat, which in maps is called *Ofila*. Damberger's fellow-travellers belonged to this nation, and resided at a place half a day's journey further to the north.

This province lies at the distance of four days' journey from the boundaries of the kingdom of Congo; it is exceedingly fertile, and abounds with productions of various kinds. The people are warlike, and defend their territories with great bravery. Our traveller was told, that they could bring into the field from 18,000 to 20,000 men; but he believes this account to be exaggerated one half, as the population appeared to be only very moderate. This nation was formerly subject to the king of Benguela, who procured from it his best soldiers; but as these soldiers were often sold for slaves when the prince stood in need of money, the people rebelled in the year 1776, and, putting to death the old king Khiguan, declared themselves independent. The son of the murdered king was afterwards placed on the throne; but in every thing that relates to public affairs he is obliged to be guided by the advice of the oldest of the inhabitants, who are assigned to him as counsellors. Disputes and quarrels among these people are settled by the oldest persons in each family; and the offending party, when the cause has been determined, is punished out of doors. Many of their customs are similar to those of the neighbouring nations, but they have some peculiar to themselves. There are fewer women here than in some of the other African nations, and it is even not uncommon for two men to have only one wife, and yet to live together in perfect harmony. The women are highly respected, and treated much better than among the surrounding nations. Male children immediately after birth are circumcised, and great rejoicings take place on such occasions, because boys are much more valued than girls. As soon as a child can speak, he is taught by his grandfather, or, if he has none, by his father, to know those vegetables and fruits which can be used as food, and to guard against those which are pernicious. He must learn also to make mats of long grass, and, when he becomes older, to take a share in the management of the domestic concerns of the family: his chief attention, however, is directed to hunting, in which it

is



is requisite that he should distinguish himself by his boldness and dexterity. When he kills an elephant, he is no longer classed among the boys, but considered as having attained to the rank of a man. Every stranger not engaged in trade with this nation is considered as a slave, but he is well treated, and never afterwards sold; for these people detest this traffic, and endeavour to free from their state of slavery those unfortunate beings who are destined by other nations to be sold. When they learn therefore that any slave-dealers are about to pass through or near their territories with a cargo of slaves, they assemble in a large body, attack the slave-merchants, and, having delivered the slaves, receive them as members of their nation. Our traveller was obliged to accompany a party of them on an expedition of this kind, but he had the misfortune to be taken prisoner. The people by whom he was made captive were called the Sovians.

Being liberated however some time after, he continued his journey on the 26th of February, 1786, and on the 5th of June following arrived at the village of Mohakam on the frontiers of the antient kingdom of Loango, where he procured a guide who conducted him to the town of Malemba, the capital of the united kingdoms of Malemba and Cacongo. This country is about four hundred miles in length, and two hundred and forty in breadth; it is exceedingly fertile, and abounds with vegetables, minerals, and animals of various kinds which are caught for the sake of their skins. The river Bambo and the lake Samoy supply plenty of fish and shellfish. The trade of this country is exceedingly advantageous to the Europeans, who for the merest trifles, such as shells and bits of iron, often get in return the best skins and furs in large quantities. The king's standing army consists of between ten and twelve thousand men, who can at any time be collected in the course of forty-eight hours. The king himself is a good warrior, leads his troops into the field in person, and is much dreaded even by his more powerful neighbours: whenever he goes abroad he is usually attended by four of his ministers, who are at the same time officers, and twelve men of his body-guard.

The town is surrounded by a wall composed of fragments  
of

of rock and loose stones, heaped together without clay or mortar, and by a ditch. The palace where the king resides is badly built, is one story high, and about five hundred feet in circumference. It stands before the water-gate on the river Malempo, towards the lake, and affords a good prospect. Our traveller saw planted near it four three-pounders.

After being exposed to various hardships among these people, our traveller, being sent out with a party to collect elephants' teeth, found means to make his escape, and arrived among a people called the Yaganese, with whom he remained till the end of the year. On the 2d of December he again resumed his journey, and, crossing the Akasi mountains, reached the territory of Mugari. This country is small, and thinly peopled. The inhabitants are of low stature, and far from being hospitable to strangers. Though of small size, they are more expert in hunting the elephant than many of their neighbours; and from the hides and teeth of these animals they procure their chief subsistence. The whole number of the inhabitants does not amount to more than three thousand: most of them live in caves of the mountains.

After passing through a variety of small villages our traveller entered the kingdom of Yukodego (Monœmugi), which towards the east borders on Abyssinia. The river Zambece, which at the town of Yukora divides itself into five branches, traverses nearly the whole of the country, and, at the distance of half a day's journey from the town of Zambre, forms a large lake, bearing the name of that town. The town of Zambre, which he visited soon after, consists of four hundred huts, and fifty or sixty houses. The former are built of timber and straw, the latter of stones and mortar. The town, which is of a triangular shape, lies on the right side of the river Zambre, has two principal and three cross streets, and is defended on one side by a wall. The houses, as well as the palace, are only one story high.

At Zambre our traveller was introduced to the king, who conducted him into what he called his hall, where he displayed to him his treasures. They consisted of a few worn copper coins, two looking-glasses, an almanack for the year 1743,  
a few



a few sheets of printed paper; four small cannons, such as children have to play with; a few other toys, and a wooden clock, which seemed to be quite deranged. The king having, by an interpreter, expressed great concern that the clock did not go as formerly, our traveller, when his majesty was gone, told the interpreter that he would try to repair it, if the king gave him permission. The king assented; and Damberger, though not much acquainted with clock-making, was so fortunate as to succeed. When the king returned next day he was much surprised, and began to entertain a very high idea of a man who had been able to effect what exceeded the skill and ability of the most ingenious of his subjects. From that moment our traveller's consequence continually increased; he received the same victuals as were used by the king, accompanied him every where, and, by means of this privilege, had an opportunity of getting a complete view of the lake of Zambre, when the king went thither to see the people employed there in fowling and fishing. This lake is in length three full days journey, and is interspersed with about forty small islands, frequented by innumerable flocks of birds. It is of an oval form, and about half a day's journey in breadth. The king keeps here a guard of two hundred men to take care that the fowling and fishing are properly conducted for his majesty's advantage; but our traveller was told that these people dress for themselves the best of the fowls and the fish that are caught.

After spending five months at this place, Damberger, having obtained the king's leave to depart, continued his journey on the 28th of May 1787, and on the 11th of June arrived at the first frontier huts of the Moohatans; a numerous and warlike nation, who inhabit a tract of land belonging to the kingdom of Monœmugi, and subject to its sovereign. As the rainy season had now commenced in this part of the country, he was obliged to remain some time under very unfavourable circumstances at the village of Mytob, from which he set out on the 19th of July, in company with some travellers from the country of Mossaguejos, lying eastward of the kingdom of Monœmugi, who were going to the kingdom of Otoba to fetch salt. After crossing the

river Druma, and ascending a high mountain to the west, he arrived among a people called the Mophanians, who dwell in caves, and by whom he was well received. The king of this country is an absolute sovereign, and possesses a tract of territory ten days journey in length from west to east, and seven in breadth from north to south. Of the various kinds of fruit-trees found here, the most remarkable are the *domo* and the *inkobak*. The former bears a kind of apples without pips, of the size of a hen's egg, having a golden colour, and a taste like that of lemons. It grows on the mountains, has long, narrow, sharp-pointed leaves, and attains to the height of the cherry-tree. The bark resembles that of the cinnamon-tree, and is preserved along with the fruit, and used all the year through as a corroborant. The *inkobak* tree bears a fruit of the nut species as large as an egg: it is of an oblong form, and has a red husk, which in all probability would afford a good colour for dyeing. Our traveller observed that the earthen-ware used by the inhabitants was dyed with it, and that it retained its beauty even in the fire. The nut itself is white, and tastes like cinnamon. The tree is as large as the oak, and all the year through bears fruit and blossoms at the same time.

After leaving these people, our author prosecuted his journey through the villages of Ohgothen, Uhroh, and Mato, to Ocymoro, the residence of the king, where he was placed among the royal slaves; but, finding means to effect his escape across the sandy deserts, he ascended the Mountains of the Moon, and arrived in the territories of the Vomanians. These people he found hospitable and good tempered; they presented him with tiger's flesh, which he had never before tasted, and, having directed him what course to proceed, wished him a prosperous journey. He now directed his course northwards across Dahamta to the kingdom of Vohyagtam; and falling in with some travelling negroes, who called themselves Taomuh, he pursued his journey in their company as far as the first huts on the frontiers of Bahura. On the 19th of November he set out with a caravan bound to Vangara, by the way of Vadgayu, Yomy, &c.; but, not being able to bear the fatigue of riding, he was left on the road, and fell sick. On his recovery, which took place



place soon after, he proceeded back again to Yandoka, and Bahahara the capital, where he was detained for a short time as a slave; but having repaired some of the king's weapons he obtained his liberty, and travelled in the suite of the king to Kahoratho.

[To be continued.]

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XII. *Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear. By Mr. ASTLEY COOPER. In a Letter to EVERARD HOME, Esq. F.R.S. by whom some Remarks are added\*.*

DEAR SIR,

AT the time you were engaged in the investigation of the structure and uses of the membrana tympani, you mentioned a wish to ascertain the effect a rupture of that membrane would have upon hearing. I now send you some observations on that subject, which, if you think them of sufficient importance, you will do me the honour of presenting to the Royal Society.

I am, &c.

ASTLEY COOPER.

ANATOMISTS have endeavoured to ascertain, by experiments on quadrupeds, the loss of power which the organ of hearing would sustain by perforating the membrana tympani: dogs have been made the subject of these trials; but the results have been neither clear nor satisfactory, and they accord but little with the phænomena I am about to relate.

Mr. Cheselden had conceived the design of making the human organ itself the subject of direct experiment; and a condemned criminal was pardoned, on condition of his submitting to it; but a popular outcry being raised, it was thought proper to relinquish the idea.

Though denied the aid of experiment, we are not without the means of obtaining knowledge upon such subjects; since the changes produced by disease frequently furnish a clue which is equally satisfactory.

It often happens, that some parts of an organ are destroyed

\* From the *Transactions of the Royal Society* for 1800.

by disease, whilst others are left in their natural state; and hence, by the powers retained by such organ, after a partial destruction, we are enabled to judge of the functions performed by those parts when the whole was in health.

Guided by this principle, I have made the human ear the subject of observation, and have endeavoured to ascertain the degree of loss it sustains in its powers by the want of the *membrana tympani*; a membrane which has been generally considered, from its situation in the *meatus*, and its connection with the adjacent parts by a beautiful and delicate structure, as essentially necessary to the sense of hearing; but which, as appears by the following observations, may be lost, with little prejudice to the functions of the organ.

Mr. P——, a medical student at St. Thomas's hospital, of the age of twenty years, applied to me, in the winter of 1797, while he was attending a course of anatomical lectures, requesting my opinion upon the nature of a complaint in his ear, which had long rendered him slightly deaf.

Upon inquiring into the nature of the symptoms which had preceded, and of those which now accompanied the disease, he informed me, that he had been subject from his infancy to pains in the head, and was attacked, at the age of ten years, with an inflammation and suppuration in the left ear, which continued discharging matter for several weeks: in the space of about twelve months after the first attack, symptoms of a similar kind took place in the right ear, from which also matter issued for a considerable time. The discharge in each instance was thin, and extremely offensive to the smell; and, in the matter, bones or pieces of bones were observable. The immediate consequence of these attacks was a total deafness, which continued for three months; the hearing then began to return, and, in about ten months from the last attack, was restored to the state in which it at present remains.

Having thus described the disease and its symptoms, he gave me the following satisfactory proof of each *membrana tympani* being imperfect. Having filled his mouth with air, he closed the nostrils and contracted his cheeks: the air, thus compressed, was heard to rush through the *meatus au-*

*ditorius*.



ditorius with a whistling noise; and the hair hanging from the temples became agitated by the current of air which issued from the ear. To determine this with greater precision, I called for a lighted candle, which was applied in turn to each ear, and the flame was agitated in a similar manner. Struck with the novelty of these phænomena, I wished to have many witnesses of them, and therefore requested him, at the conclusion of the lecture upon the organ of hearing, to exhibit them to his fellow students; with which request he was so obliging as to comply.

It was evident from these experiments, that the membrana tympani of each ear was incomplete, and that the air issued from the mouth, by the Eustachian tube, through an opening in that membrane, and escaped by the external meatus.

To determine the degree in which the membrana tympani had been injured, I passed a probe into each ear, and found that the membrane on the left side was entirely destroyed; since the probe struck against the petrous portion of the temporal bone, at the interior part of the tympanum, not by passing through a small opening; for, after an attentive examination, the space usually occupied by the membrana tympani was found to be an aperture, without one trace of membrane remaining.

On the right side, also, a probe could be passed into the cavity of the tympanum; but here, by conducting it along the sides of the meatus, some remains of the circumference of the membrane could be discovered, with a circular opening in its centre, about the fourth of an inch in diameter.

From such a destruction of this membrane, partial indeed in one ear, but complete in the other, it might be expected that a total annihilation of the powers of the organ would have followed: but the deafness was inconsiderable. This gentleman, if his attention were exerted, was capable, when in company, of hearing whatever was said in the usual tone of conversation; and it is worthy of remark, that he could hear with the left ear better than with the right, though in the left no traces of the membrana tympani could be perceived.

When attending the anatomical lectures, also, he could hear,

hear, even at the most distant part of the theatre, every word that was delivered; though, to avoid the regular and constant exertion which it required, he preferred placing himself near the lecturer.

I found, however, that when a note was struck upon the piano forte, he could hear it only at two-thirds of the distance at which I could hear it myself; and he informed me, that in a voyage he had made to the East Indies, while others, when ships were hailed at sea, could catch words with accuracy, his organ of hearing received only an indistinct impression. But the most extraordinary circumstance in Mr. P——'s case is, that the ear was nicely susceptible of musical tones; *for he played well on the flute, and had frequently borne a part in a concert.* I speak this, not from his own authority only, but also from that of his father, who is an excellent judge of music, and plays well on the violin: he told me, that his son, besides playing on the flute, sung with much taste, and perfectly in tune.

The slight degree of deafness of which Mr. P. complained, was always greatly increased by his catching cold: an effect which seems to have arisen from the meatus being closed by an accumulation of the natural secretion of the ear; for it frequently happened to him, after he had been some time deaf from cold, that a large piece of hardened wax, during a fit of coughing, was forced from the ear, by the air rushing from the mouth through the Eustachian tube, and his hearing was instantly restored.

From bathing, likewise, he suffered considerable inconvenience, unless his ears were guarded against the water, by cotton being previously forced into the meatus. When this precaution was neglected, the water, as he plunged in, by rushing into the interior parts of the ears, occasioned violent pain, and brought on a deafness, which continued until the cause was removed, that is, until the water was discharged: but he had acquired the habit of removing it, by forcing air from the mouth through the ear.

In a healthy ear, when the meatus auditorius is stopped by the finger, or is otherwise closed, a noise similar to that of a distant roaring of the sea is produced: this arises from



the air in the meatus being compressed upon the membrana tympani. In the case here described, no such sensation was produced: for, in Mr. P.'s ear, the air, meeting with no impediment, could suffer no compression; since it found a passage, through the open membrane, to the mouth, by means of the Eustachian tube.

Mr. P. was liable to the sensation commonly called the *teeth being on edge*, in the same degree as it exists in others; and it was produced by similar acute sounds, as by the filing of a saw, the rubbing of silk, &c. Its occurring in him seems to disprove the idea which has been entertained of its cause; for it has been thought, that the close connection of the nerve called the corda tympani with the membrana tympani, exposed it to be affected by the motions of the malleus; and that, as it passes to nerves connected with the teeth, they would suffer from the vibratory state of the nerve, produced by the agitations of the membrane. But, in this case, as the membrane was entirely destroyed on that side on which the sensation was produced, some other explanation must be resorted to; and I see no reason why this effect should not be referred to that part of the auditory nerve which lines the labyrinth of the ear, which, being impressed by acute and disagreeable sounds, would convey the impression to the portio dura of the same nerve, and to the teeth with which that nerve is connected.

The external ear, though two distinct muscles are inserted into it, is capable, in its natural state, of little motion: however, when an organ becomes imperfect, every agent which can be employed to increase its powers is called into action; and, in the case here described, the external ear had acquired a distinct motion upward and backward, which was observable whenever Mr. P. listened to any thing which he did not distinctly hear. This power over the muscles was so great, that when desired to raise the ear, or to draw it backwards, he was capable of moving it in either direction.

This case is not the only one of this description which has come under my observation; for another gentleman, Mr. A., applied to me under a similar complaint, (but in one ear only,) proceeding from suppuration, and producing the same effects.

effects. This gentleman has the same power of forcing air through the imperfect ear; suffers equally from bathing, if the meatus auditorius be unprotected; and feels, even from exposure to a stream of cold air, very considerable pain. The only difference I could observe was, that in Mr. A.'s case, the defect of hearing in the diseased organ was somewhat greater than in the former; for though, when his sound ear was closed, he could hear what was said in a common tone of voice, yet he could not distinguish the notes of a piano forte at the same distance: a difference which might have in part arisen from the confused noise which is always produced by closing the sound ear; or because, as he heard well on one side, the imperfect ear had remained unemployed, and consequently had been enfeebled by disuse.

From these observations it seems evidently to follow, that the loss of the membrana tympani in both ears, far from producing total deafness, occasions only a slight diminution of the powers of hearing.

Anatomists who have destroyed this membrane in dogs have asserted, that at first the effect on the sense of hearing was trivial; but that, after the lapse of a few months, a total deafness ensued. Baron Haller also has said, that if the membrane of the tympanum be broken, the person becomes at first hard of hearing, and afterwards perfectly deaf. But, in these instances, the destruction must have extended further than the membrana tympani; and the labyrinth must have suffered from the removal of the stapes, and from the consequent discharge of water contained in the cavities of the internal ear; for it has been very constantly observed, that when all the small bones of the ear have been discharged, a total deafness has ensued.

It is probable, that in instances in which the membrana tympani is destroyed, the functions of this membrane have been carried on by the membranes of the fenestra ovalis and fenestra rotunda: for, as they are placed over the water of the labyrinth, they will, when agitated by the impressions of sound, convey their vibrations to that fluid in a similar manner, though in somewhat an inferior degree, to those which are conveyed by means of the membrana tympani and the  
small



small bones which are attached to it ; and thus, in the organ of hearing, each part is admirably adapted, not only to the purpose for which it is designed, but also as a provision against accident or disease ; so that, whenever any particular part is destroyed, another is substituted for it, and the organ, from this deprivation, suffers but little injury in its functions.

It seems that the principal use of the membrana tympani is, to modify the impressions of sound, and to proportion them to the powers and expectation of the organ. Mr. P. had lost this power for a considerable period after the destruction of the membrane ; but, in process of time, as the external ear acquired the additional motions I have described, sounds were rendered stronger or weaker by them. When, therefore, he was addressed in a whisper, the ear was seen immediately to move ; but, when the tone of voice was louder, it then remained altogether motionless.

*Some additional Remarks, on the Mode of Hearing in Cases where the Membrana Tympani has been destroyed. By*  
EVERARD HOME, Esq.

After having communicated to this learned Society the very curious facts contained in Mr. Cooper's paper, which prove that the organ of hearing is capable of receiving all the different impressions of sound, when the membrana tympani has been destroyed, it may not be improper to explain, from the observations contained in a former paper on this subject, in what manner this may take place.

It is there stated, that any vibrations communicated directly to the bones of the skull, are as accurately impressed upon the organ, as through the medium of the membrana tympani. The office of that membrane is therefore to afford an extended surface, capable of receiving impressions from the external air, and of communicating them to the small bones of the ear ; which a membrane would be incapable of doing, unless it had a power of varying its tension, to adapt it to different vibrations.

In the above cases, in which this membrane, the malleus, and the incus, had been destroyed, it would appear that the stapes was acted upon by the air received into the cavity of



the tympanum, and communicated the impressions immediately to the internal organ. This not happening for some months after the membrane was destroyed, probably arose from the inflammation of the tympanum confining the stapes, and rendering its vibrations imperfect.

That sounds can be communicated with accuracy by the bones of the skull to the internal organ, when received from solid or liquid substances, has long been well understood.

That the membrana tympani is incapable of perfectly answering this purpose, when sounds are propagated through air, has been a generally received opinion; to refute which, was the object of my former paper. That, in cases in which the membrana tympani has been destroyed, the air is capable of acting with sufficient force upon the stapes to communicate vibrations to it, and to produce on the internal organ the necessary effect for perfect hearing, is completely ascertained by Mr. Cooper's observations.

XIII. *Analysis of a Stone called the Gadolinite; with an Account of some of the Properties of the new Earth it contains.* By C. VAUQUELIN\*.

THE number of simple bodies, and particularly that of earths, has been greatly increased within a few years past; and if chemistry go on still thus advancing, it is to be apprehended that the time may arrive when the human mind will not be able to embrace all the combinations produced by the multitude of simple bodies.

But the analogy which exists between certain classes of natural substances, gives chemists reason to suspect that they contain a common generating principle, and affords them a hope that some fortunate chance, or an experiment made by some man of genius, will reduce them all to one single kind, either by disengaging them from those principles which establish differences between them, or by combining these same elements to those which are simple.

\* From the *Annales de Chimie*, No. 107.



Till chemical means are brought to that degree of perfection, we must make known those bodies which, on account of their peculiar properties, we cannot refer to any of those already known, and which, till a new order be established, we are obliged to consider as new substances. An attentive examination of their properties is of great importance; for, even though they may be only modifications of bodies already known, it may happen, of which we have many instances, that their qualities may become useful to the arts and to manufactures, and, in any event, it is better to err through excess than deficiency.

With this view I shall here describe, with some details, the properties of a new earth discovered by M. Gadolin, and which has been examined by M. Ekeberg under new points of view.

I shall first give a short description of the stone, and then explain the different processes which I employed to analyse it; and shall conclude with indicating the principal properties that characterise the new earth it contains. The greater part of the observations I shall make on this subject, may have been already published by the chemists above mentioned; but their works being not much known, I have thought it my duty to act in this manner, that the present discovery may be better propagated.

I shall, however, repeat what has been communicated to me by M. Manthey, professor of chemistry at Copenhagen, to whom both Haüy and I are indebted for a large quantity of matter containing this new earth\*.

In 1794, M. Gadolin discovered this earth; and his labour on the subject was printed in the *Memoirs of the Academy of Sweden*, and in *Crell's Chemical Annals* for the year 1796. M. Ekeberg, about two years ago, began an analysis of the same stone, and confirmed the results of M. Gadolin. To the new earth he gave the name of *Yttria*, from Ytterley, a place in Sweden, where it is found. A short mention of it was made also in the *Annales de*

\* Professor Abildgaard, also, has had the goodness to send me a pretty large quantity of this stone, which enabled me to vary my experiments, and to discover the characterising properties of the new earth it contains.

*Chimie*, No. 100. This earth, according to him, exists in the gadolinite in the proportion of

		0.47
Accompanied by flix	—	0.25
Oxyd of iron	—	0.18
Alumine	—	0.04

He describes also some of the properties possessed by this new earth when freed from all the bodies united to it in the stone, and which are as follows:—"All its combinations with acids have a sweet taste like that of the salts of lead, but a little more astringent; with the sulphuric and acetic acids it forms crystallisable salts which do not change in the air; with nitric acid it gives a radiating mass, and with the muriatic acid nothing that can crystallise.

#### *Characters of the Stone.*

1st, This substance has a black colour, and its dust is of a blackish gray.

2d, Its fracture is absolutely vitreous, like that of glass.

3d, Its specific gravity, ascertained by Haüy, is 4.0497.

4th, It makes the magnetic needle move in a sensible manner.

5th, When exposed to the blow-pipe, it splits into small fragments, which fly to a considerable distance in bright red sparks, which, when they detach themselves, produce a strong crackling noise. What remains of the stone has a grayish-white colour, and does not fuse completely.

6th, When heated with borax it fuses, and communicates to that salt a yellow colour inclining to violet.

7th, A hundred parts of this substance, exposed to heat in a platina crucible, lose eight parts of their weight, and the matter assumes an ochry red colour. If, from the quantity of iron it contains, we estimate the oxygen it must absorb by the operation, we shall find that it has lost about 11 per cent.

#### *Phænomena which the Gadolinite exhibits with the Mineral Acids.*

The gadolinite (it is by this name I shall distinguish this stone in the course of this memoir,) is attacked by the powerful mineral acids, such as the sulphuric, the nitric, and the muriatic;



muriatic; and, if their action be assisted by a gentle heat, they form a thick jelly of a grayish or yellowish colour. If this kind of jelly be then evaporated to dryness, and the residuum of the evaporation be washed in water, you will obtain filex under the form of a white powder, which, when well washed and brought to a red heat, gives by its weight the proportion in which it exists with the other principles.

The solutions of gadolinite in acids do not all exhibit the same phænomena by evaporation. The sulphuric and muriatic acids retain the iron and the new earth in combination, and nothing but the filex is separated; while, on the contrary, the nitric acid abandons at the same time the filex and the oxyd of iron, which may be easily conceived from the properties of the nitrat of iron.

I took advantage of this property in preference to any other, in order to apply it to the analysis in question.

I dissolved 100 parts of gadolinite in nitric acid sufficiently diluted with water, and subjected it to evaporation, exposing it to a little heat towards the end, to effect a complete decomposition of the nitrat of iron. By re-dissolving it in water, I obtained, combined with the nitric acid and dissolved, the peculiar earth separated from the iron and the filex. While my solution still retained some traces of iron, which I easily perceived either by the reddish colour or by the gallic acid, I again evaporated the liquor to dryness, or I added a drop of ammonia, and the iron was then precipitated under the form of yellowish flakes, which I separated by the filter.

To separate the iron from the filex, I boiled the mixture in muriatic acid a little concentrated; I then diluted the solution with water, and made it pass through the filter, in order that I might collect the filex, and wash it till it was no longer precipitated by ammonia.

In regard to the new earth dissolved in nitric acid, it will be sufficient if it be pure to precipitate it by ammonia, and to wash it to obtain it separate; but having learned, by preliminary trials, that in this state it is mixed with small quantities of lime and magnesia, I was obliged to employ some further means for accomplishing that end.

I never-

I nevertheless began to precipitate it by means of ammonia, which does not precipitate lime. I then poured into the liquor, united to the washings of the precipitate, some drops of a solution of common carbonat of potash, and I obtained the lime combined with the carbonic acid. I redissolved, for the third time, in nitric acid, the earth mixed with the oxyd of manganese, and added, in small quantities at a time, a solution of hydro-sulphuret of potash, in order that I might precipitate only the metallic parts, which, with a little attention, I was able to effect.

I had then the earth alone, so that nothing was necessary to obtain it pure but to precipitate it by ammonia.

*Analysis of the Gadolinite by Potash.*

I still employed another method, which also succeeded, to separate the different elements which constitute the gadolinite: it consists in fusing it with two parts of caustic potash, washing the mass with boiling water, and filtering the liquor, which has a beautiful green colour.

In evaporating this liquor, the manganese, which gave it its colour, was gradually precipitated under the form of a black powder, which could be easily collected by decanting the supernatant water.

When it is observed that there is no more oxyd of manganese, the liquor must be saturated with nitric acid: on the other hand, the sediment must be put to digest with the same acid much diluted with water: by these means the earth alone dissolves, producing a great deal of heat; and the flint; and the iron too much oxydated, do not dissolve.

This solution with the above liquor, saturated with nitric acid, must be evaporated to dryness, in order that if any parts of iron and flint have remained they may be separated: in other respects the process is the same as before. The latter has the advantage of separating the manganese from the other principles, and of rendering unnecessary an operation the success of which is difficult.

The gadolinite might also, rigorously speaking, be analysed by directly attacking it by the sulphuric and muriatic acids; but as these acids dissolve at the same time, and

without



without distinction, all the elements which compose this stone, a hydro-sulphuret must be employed to separate the metals; and the just management of this operation is difficult, because the new earth is precipitated by an excess of the re-agent.

By the help of those means, which I have briefly explained, I was able to discover and separate the substances which enter into the composition of the stone called *gadolinite*. These substances are siliceous, black oxyd of iron, lime, oxyd of manganese, and the peculiar earth to which M. Ekeberg has given the name of *Yttria*.

The proportions in which they are found are as follow :

1st, Siliceous	—	—	25.5
2d, Oxyd of iron	—	—	25
3d, Oxyd of manganese	—	—	2
4th, Lime	—	—	2
5th, The new earth, or <i>Yttria</i>			35
			<hr/>
			89.5
		Loss	10.5

These 10.5 form the smallest loss I experienced in the different analyses I made; for I thought it necessary to employ, in constructing this table, the greatest quantities of each of the matters obtained by either of the processes.

I at first thought that this loss arose from some alkaline substances, as is the case in several kinds of stone; but, having treated 100 parts by sulphuric acid, I assured myself that it arose from another cause; for, having precipitated by ammonia all the earthy and metallic matters dissolved in the sulphuric acid, and having brought to a red heat the salt produced by evaporating the liquor to dryness, there remained nothing in the crucible but a little sulphat of lime.

As I suspected that this was occasioned by some volatile substance, I heated, in a platina crucible, 100 parts of the stone reduced to powder, and found that it had decreased eight parts, and that the remainder had assumed a yellowish colour.

The slight effervescence which I had always remarked

when the stone was dissolved in acids, induced me to imagine that one part at least of the deficit was owing to the carbonic acid.

To assure myself of it, I introduced into a small phial 100 parts of the matter in powder, and, after preparing a glass tube, destined to convey the gas into the lime-water, I poured into it sulphuric acid diluted with a certain quantity of water; a swelling up and heat were produced, and some air-bubbles passed into the lime-water, by which it was rendered turbid; but the quantity of the precipitate was so small, that it was impossible for me to measure it. As the space, indeed, in the vessels which remained empty was so great, it is probable the greater part of the carbonic acid remained in it.

But this experiment sufficiently proved to me that the 10.5 of loss belonged not merely to the carbonic acid; for, though the space furnished by my vessels was pretty considerable, it would not have been capable of retaining it, and I should also have obtained a greater quantity of calcareous precipitate. In the hope of finding some other substance, which, with carbonic acid, might contribute to form this loss, I put 100 parts in a retort of luted glass, to which I adapted a small receiver, and I exposed it to a strong heat. There appeared in the neck of the retort, and even in the bottle, some small drops of water, the quantity of which was so small that I could not weigh it. But the matter taken from the retort weighed no more than 91 parts. Thus it appeared to me that the loss which I experienced in my analyses is chiefly owing to water and carbonic acid.

Having given the most remarkable characters of the fossil called *gadolinite*, and the processes which appeared to me best adapted for separating its principles, I shall now briefly explain some of the properties exhibited by the new earth extracted from it.

1st, It is perfectly white; but it is pretty difficult to obtain it in that state on account of the oxyd of manganese, which follows it in almost all its combinations.

2d, It has neither flavour nor smell.

3d, It



3d, It is not fusible alone, but with borax it forms a white glass, which is transparent when it has not been added in excess.

4th, It is not sensibly soluble in caustic fixed alkalies: in this it is different from alumine and glucina, which combine with these very easily and in large quantity.

5th, It is soluble in the carbonat of ammonia, but five or six times less so than glucina; that is to say, five or six times more carbonat of ammonia is required to dissolve an equal quantity of yttria.

6th, It combines rapidly with the sulphuric acid, and produces heat in proportion as the union is effected: the salt thence resulting crystallises in small brilliant grains little soluble in water: it appeared to me that more than fifty parts of cold water were necessary to dissolve it, especially when not accompanied with an excess of acid. It has a taste at first astringent, and afterwards sweet; like sugar or salt of lead. This property, though analogous to that of glucina, is, however, so sensibly different from it, that by comparing them they may be easily distinguished.

7th, Its combination with the nitric acid has a more striking favour, but it produces in the mouth an effect of the same nature: it crystallises only with difficulty, and its affinity for water is so great, that it requires some trouble to dry it. During this operation, if it be exposed to too much heat, instead of becoming solid, like the greater part of the salts, it grows soft, and assumes the appearance of thick transparent honey; by cooling, it becomes hard and brittle like a stone; when exposed to the air, it attracts the moisture of it, and becomes soft.

The sulphuric acid, poured into a solution of the nitrat of yttria, forms in it a crystalline precipitate, which is a sulphat of the same earth.

8th, The combination of this earth with the muriatic acid exhibits nearly the same phænomena, in the several experiments I made, as the nitrat above examined: like the nitrat it can be dried with difficulty, it is fusible by a gentle heat, and strongly attracts the moisture of the atmosphere.

9th, Ammonia precipitates yttria earth from the three

combinations above mentioned; lime and barytes produce much more sensibly the same effect.

10th, The oxalic acid, and consequently the oxalat of ammonia, form in its solutions precipitates which have an appearance absolutely similar to that of the muriat of silver: glucina with the oxalic acid forms a very soluble salt—a new difference between these two earths.

11th, Prussiat of potash crystallised and re-dissolved in water occasions, in the solutions of this earth by acids, a white granulated deposit; but this is not the case in solutions of glucina.

12th, The phosphoric acid does not precipitate it from the other acids, but the phosphat of soda separates it under the form of gelatinous white flakes.

13th, It appears to me that it has more affinity for, at least, some of the acids, than glucina has.

14th, It precipitates an infusion of gall-nuts in brown flakes.

From what I have said, a great number of analogies may without doubt be observed between this earth and glucina; but, at the same time, there appear differences which do not permit us to confound these two earths. These differences chiefly are, the insolubility of the yttria and the solubility of glucina in fixed caustic alkalies; the little solubility of the sulphat of the yttria, and the great solubility of glucina, in the sulphuric acid; the difficult solubility of yttria, and the ready solubility of glucina, in carbonat of ammonia; the precipitation of yttria, and non-precipitation of glucina, from their solutions by oxalic acid and the prussiat of potash.

Here then we have nine kinds of earth very distinct by the properties peculiar to each: soon, no doubt, we shall reckon ten, if, as we have reason to presage from the accuracy of M. Tromsdorf, the existence of that which he has lately announced under the name of *august*, in the Saxon beryl, be realised.

These earths will increase in a wonderful manner the number of the saline combinations, which are already very considerable, and will furnish to chemists a multitude of new properties to be studied. It is to be wished that they may find



find some of them applicable to the arts, in order that these discoveries may not remain altogether useless. It is to be wished also, when the properties of this new earth shall be better known, that chemists may give themselves the trouble to change the name of yttria, by which it is now known, and which is derived from Ytterley, the place where it is found, that another derived from its essential properties may be given to it.

I shall conclude with remarking the great difference between the result of M. Ekeberg's analysis and mine. I do not know exactly to what it is owing, but I can assert that, in five analyses which I made of this stone by different processes, I never had less than 12 of loss. I presume that a certain quantity of moisture, and perhaps carbonic acid, remained in the new earth obtained by M. Ekeberg, for it is chiefly in this point that we differ. He found 47.5, and I only from 34 to 35, because I calcined it more strongly.

## NEW PUBLICATIONS.

*A Manual of a Course of Chemistry; or, a Series of Experiments and Illustrations necessary to form a complete Course of that Science.* By J. B. BOUILLON LAGRANGE, Professor in the Central Schools of Paris, &c. Translated from the French, with 17 Plates: 2 Vols. 18 Shillings. Cuthel, and Vernor and Hood, 1800.

[Continued from Page 282.]

IN our last we announced the publication of this useful work, and promised a few extracts. We now subjoin the following.

### *Citric Acid [Concrete juice of lemons].*

Scheele was the first person who found means to obtain the citric acid crystallized, and well separated from the mucilage, which accompanies it in the juices of those fruits that furnish it.

The process for obtaining this acid, according to that chemist, is as follows: Express the juice of lemons, and leave it

at rest for twenty-four hours, to favour the separation of the mucilage; then filter it through paper, and saturate it with a quantity of the carbonate of lime. The citrate of lime, which results from this saturation, being insoluble, is precipitated to the bottom of the liquor: when this deposit is well formed, draw off the supernatant liquor, and wash the precipitate until it becomes insipid and exceedingly white; then decompose this salt by the help of a gentle heat, with half its weight of sulphuric acid, diluted with six parts of water; the sulphuric acid takes the lime from the citric acid; the greater part of the sulphate of lime formed is precipitated, and the citric acid remains free in the water. This acid may be obtained under a crystalline form, by evaporating it to the consistence of clear syrup, and then suffering it to cool.

Dizé, who made several experiments on this subject, found that an excess of sulphuric acid was necessary to destroy the portion of mucilage, which the acid obstinately retains in its combination with the lime, and which opposes its crystallization. He observed also, that to obtain the citric acid perfectly pure, it was necessary to dissolve it, and to cause it to crystallize several times.

The crystals obtained by Dizé were rhomboidal prisms, having their planes inclined to each other at angles of about 60 or 120 degrees, and terminated on each side by summits with four faces, which intercepted the solid angles.

One part of distilled water at the temperature of ten degrees, according to this author, dissolves 1.25 of crystallized citric acid, and, during the solution, cold equal to 13 degrees (29° F.) is produced.

This acid reddens blue vegetable colours: when exposed to the fire in close vessels, with a pneumatic apparatus, it is decomposed; an acid phlegm, carbonic acid gas, and carbonated hydrogen gas, are obtained from it: and a little charcoal remains.

Its crystals effloresce in the air.

An agreeable lemonade may be prepared with this acid: for this purpose Dizé proposes about half a dram of the acid dissolved in about two pounds of water, and a sufficient quantity of sugar and *oleosaccharum*, made with lemon peel.



If a bit of sugar be rubbed against a piece of orange or lemon-peel, it will imbibe the volatile oil, and form an oleo-saccharum, soluble in water, and exceedingly proper for rendering certain liquors aromatic.

[To be continued.]

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MR. BLAIR, of the Lock Hospital and Finsbury Dispensary, assisted by several other respectable surgeons, has been some time engaged in writing a comprehensive *System of medical and operative Surgery*, adapted to the present improved practice at the London Hospitals, &c.

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MR. WILLIAM HENRY, of Manchester, has in the press, and in considerable forwardness, a small work, intended, partly, to facilitate the acquirement of chemical knowledge, to persons entering on the study, without the benefit of an instructor; and, partly, as a pocket companion, for the use of more advanced students. The first part will contain directions respecting the best mode of studying chemistry; and also, an arranged series of experiments, necessary to be performed by those who intend to become acquainted, by actual observation, with the chemical properties and habits of bodies. More minute directions will be given, for conducting these experiments with success, than are to be found in other elementary books. The second part will comprise summary instructions respecting the analysis of mineral waters, and of mineral bodies in general: and the third part will point out some of the useful applications of chemical agents, in detecting adulterations, in discovering poisons, &c. The work will form one small pocket volume; and it may be proper to observe, that it will not at all interfere with the excellent little manual, lately published by Mr. Parkinson, the plan and objects of which are perfectly different.

# INTELLIGENCE,

## AND

### MISCELLANEOUS ARTICLES,

---

#### ROYAL SOCIETY OF LONDON.

**T**O our present number we have prefixed a striking likeness of the Right Hon. and learned President of this Society, a gentleman whose unwearied zeal and princely liberality in promoting every thing that can advance science or add to the comforts of mankind, place him far above any eulogy in our power to bestow. We only speak the sentiment of the whole philosophical world, when we express our ardent wish that he may long continue to preside over that learned Body, to which the world owes so many discoveries, and which has made such rapid advances since this Right Hon. Gentleman was called to the chair he so ably fills.

On the 8th of January the Society met for the first time since the Christmas recess, when the conclusion of Doctor Young's paper on the mechanism of the eye was read.

An appendix by Dr. Herschel, to his paper on the power of penetrating space by means of telescopes, was read. The means extend so far as to discover nebulae, the light of which, according to the velocity ascribed to that fluid, must have been one million nine hundred and ten thousand years coming to this earth!

A paper on impossible quantities, by Mr. Woodhouse, was read the same evening.

Jan. 15, a letter by Mr. William Hay was read, giving an account of a ship springing a leak in the Indian seas. The water, which gained so fast as to render it impossible, by pumps and the usual means, to prevent the ship from sinking, was fortunately stopped by pouring rice into that part of the ship. The rice in four days dissolved, and in about eight days after became so hard that with difficulty



it could be chiseled out when the ship came into port. The author states, that the same means have been tried at other times, but not with the same success, and surmises that heat is necessary.

On the 22d was read a paper on the production of cold, by means of muriat of lime, by Mr. Walker.

The first consul of France transmitted to the Society "Voyage autour du Monde, par C. E. Marchand," 4 tom. most handsomely bound. The thanks of the Society were ordered to be returned to the first consul of France for this present.

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METEOROLOGY.

The following particulars respecting the storm on the 9th of November, furnished by an intelligent correspondent, cannot but prove interesting to some of our readers :

The preceding night had been stormy, with torrents of rain. At 9 A. M. wind abated and rain over. Thermometer 55°. Barometer, descending  $\frac{1}{5}$  inch *per* hour, gave evident intimation of the event. About noon it had arrived at its lowest point, having dropped from 29,9 to 28,5 since the morning of the sixth. At this time it was cloudy and nearly calm, when the wind veered to the northward, and in a few minutes blew from north-west with astonishing violence for about an hour. The quicksilver, which had scarcely become stationary, as suddenly changed its direction, and continued to rise at the rate of  $\frac{2}{10}$  *per* hour the whole afternoon. It became stationary at 29,7, with a hoar-frost in the morning. The index-hand of an excellent wheel barometer, fixed against a solid wall, was observed to librate continually during this rapid ascent, retreating about  $\frac{1}{100}$ , and then advancing a little more. In the space of about six hours, on this occasion, the atmosphere passed through near one-half of its total variation of gravity, which would require, in its ordinary course in this country, about as many days.

This impetuous irruption of dense air from the northern regions was probably occasioned by the sudden rarefaction consequent upon a general precipitation of that water which  
during

during the preceding four weeks had been accumulating in the atmosphere in a dissolved state. The loss, in a certain space of atmosphere, of so great a quantity of elastic fluid, and the operation of its disengaged caloric on the remainder, will at once account for the effect to those who are acquainted with pneumatics.

#### THE NEW ALKALI.

In our last (p. 291) we mentioned that a Mr. Hahnemann, of Altona, had announced the discovery of a new alkali, to which he gave the name of *pneum*, from its property of swelling by heat to twenty times its original volume. Both Klaproth and Hermstadt have lately examined this alkali, and found it to consist of nothing but *borax*.

#### ALKALINE PRÜSSIAT.

Dr. A. N. Scherer has (in his journal) recommended to chemists to take up the idea suggested by Scheele, and to endeavour, by following the hint given by that author, to produce a perfectly pure alkaline prussiat. Scheele states that prussiat of mercury is perfectly free from iron, and that, were a combination of that kind decomposed by an alkali, the result would certainly be prussiat of alkali entirely free from iron.

#### A NEW EARTH.

The particular properties of a new species of earth called *Ytria*, of which an account was given by Ekeberg in the third volume of Scherer's journal, have been confirmed by Klaproth, who lately read a paper on this subject at the Royal Academy of Berlin. Vauquelin has also analysed the stone which produces this earth.—See his Memoir on this substance in the present number, p. 366.

#### CHEMICAL EXPERIMENTS ON THE MATTER OF BLACK VOMIT.

A memoir on the analysis of black vomit, by Dr. Cathrall, was read before the American Philosophical Society at Philadelphia, on June 20, 1800. This is a very interesting and instructive paper. The experienced and intrepid author has given a description of the black vomit, has analysed the fluids ejected a few hours before the commencement of black vomiting,



miting, and exhibited a set of experiments on the matter of black vomit itself: to which he has added, experiments to ascertain the effects of black vomit on the living system of man and other animals, and a synopsis of the opinions of authors concerning its formation and qualities. The experiments show that this singular morbid excretion contains an acid, which is neither carbonic, phosphoric, nor sulphuric; and, what our readers will hardly expect, that the black vomit may be smelled, tasted, and swallowed, without inducing yellow fever, or even any sickness at all: so little infection or contagion does it seem to contain! He concludes it to be an altered secretion from the liver.

TETANUS CURED BY THE COLD BATH:

*Communicated in a Letter to Dr. R. H. ARCHER, of Baltimore, by Dr. WILLIAM HARRIS, of Pennsylvania\*.*

In the autumn of 1799, I visited a labourer, about thirty years of age, of a slender make, but healthy, who was suddenly seized, whilst in bed, with spasms in his lower extremities, which shortly after affected his whole system, but particularly his stomach, which was drawn in a hard lump, and protruded to a considerable distance. His pains were excruciating. He had a violent vomiting and purging, which came on an hour after seizure, and continued about two hours. At one time he had emprosthotonos, at another opisthotonos, to the greatest degree, and sometimes complete tetanus. The muscles of his face were drawn in every direction, and deglutition entirely impeded. His pulse varied much, but was generally feeble. He could assign no cause for the attack. I bled him, put him in the warm bath, and used all the remedies laid down by medical writers, but without any mitigation of his pains, or relief to his spasms.

At this time, which was twenty hours after the attack, when the cold sweat of death appeared to be upon him, his tongue had refused its office, his eyes sunk, having a glassy appearance, and his exit was every moment expected, it occurred to me that the cold bath might have a good effect; and, after consulting his friends, who readily acquiesced, I

\* *Medical Repository*, vol. iv. p. 76.

had him, in this state, carried in a blanket to a forge-dam which was at hand, and plunged in. He was then insensible. His spasms immediately abated, and, in twenty minutes, totally ceased. The debility induced by muscular exertion was such that it required several days before he could be removed; after which he rapidly recovered, and is at this time perfectly well.

This was a complete tetanus; and, I think, tetanus from wounds, &c. would yield to the same mode of treatment.

#### TREATMENT OF LOCKED-JAW BY ELECTRICITY.

Dr. Samuel Perry, of New-Bedford [*America*], has communicated to the public the successful result of two experiments, in curing the locked-jaw by means of electricity. Previous to the application of the electrical fluid, recourse had been had to bleeding, cathartics, antispasmodics, the warm bath, and opium applied internally and externally, without the least effect in either case. But a small receiver being filled, and discharged through the jaws of the persons affected, they flew open instantaneously. In one case the complaint was entirely removed by three shocks; in the other, by an occasional shock for a few days. Both the patients were strong and healthy persons, the one a man and the other a woman, and the mode of treating them had been similar.

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Dr. PEARSON'S Lectures on the Materia Medica, Practice of Physic and Chemistry, will recommence at the Elaboratory, Whitcomb-street, Leicester-square, on Tuesday, February 3, at eight in the morning.



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#### ERRATUM.

Page 8, line 12, for *rubbed out*, read *oiled out*.

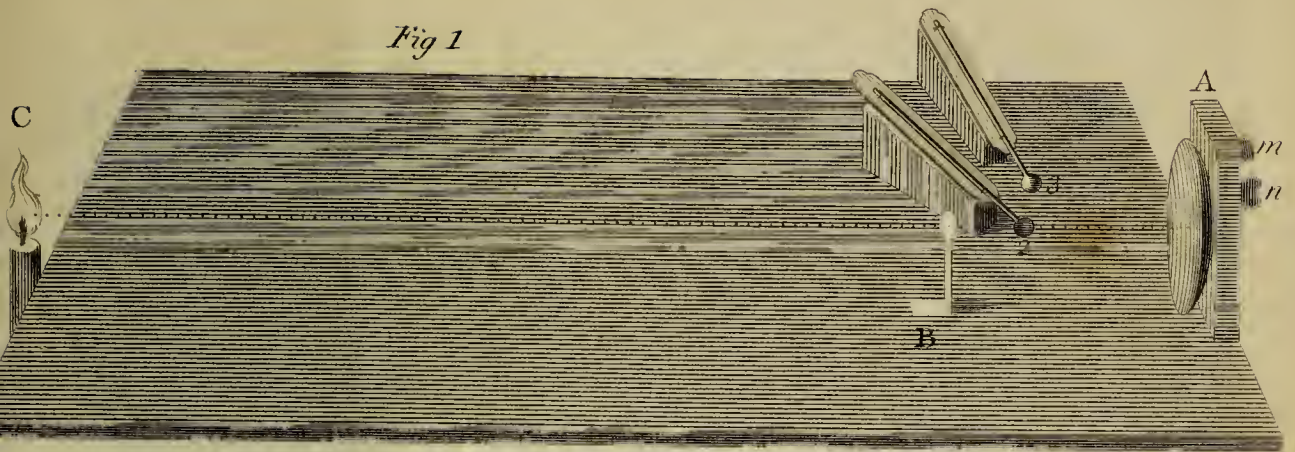
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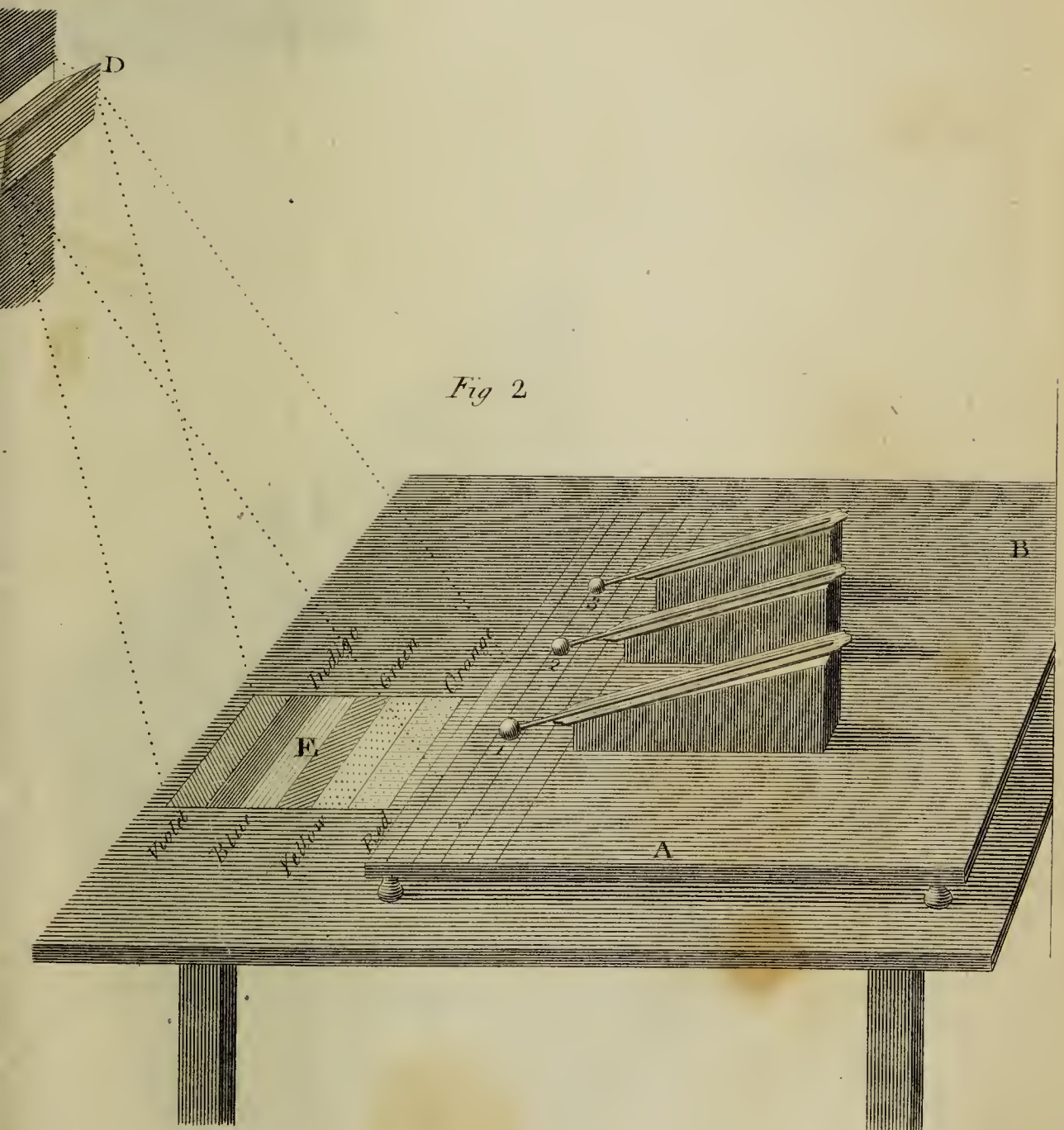
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*Fig 1*



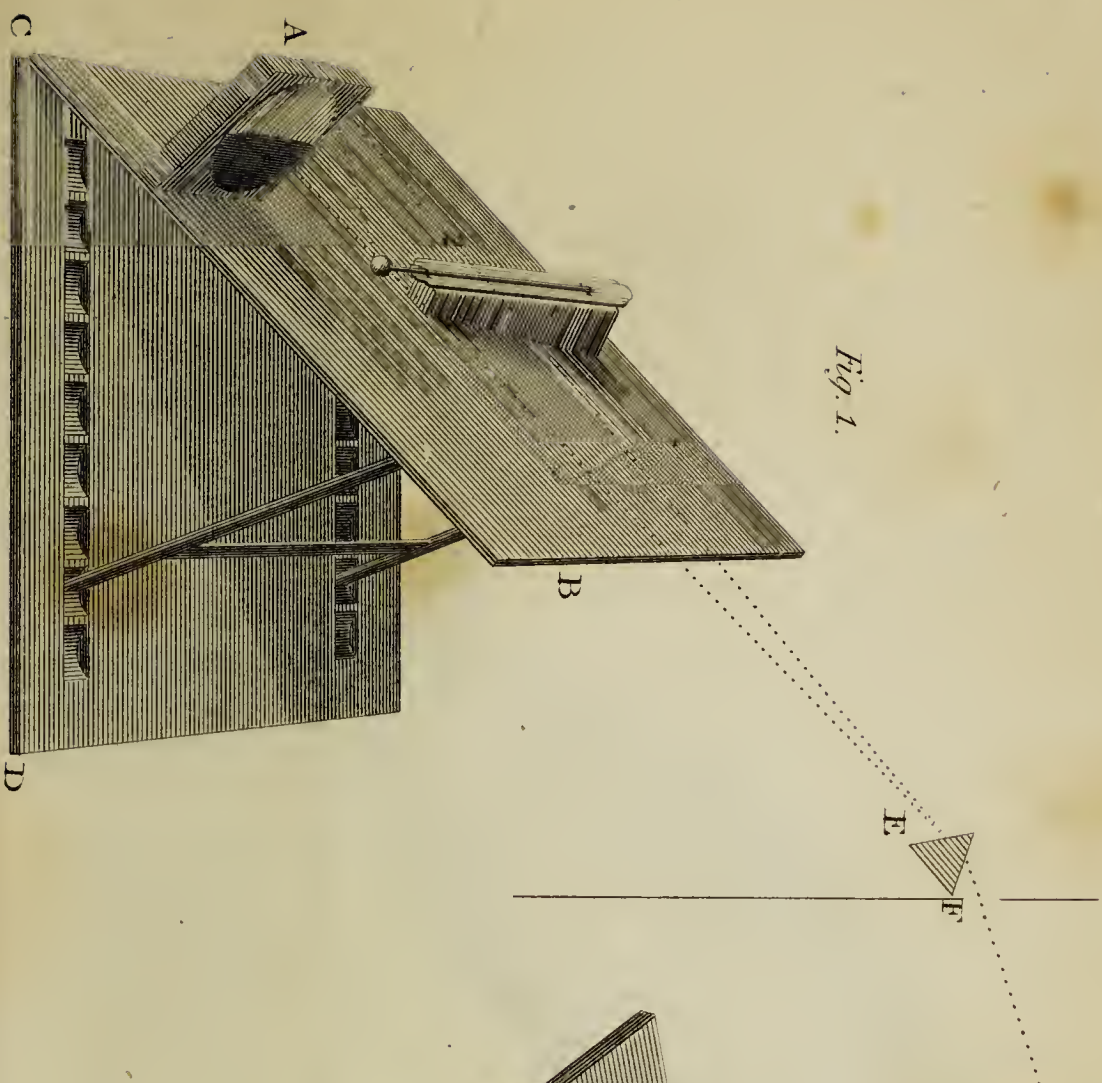
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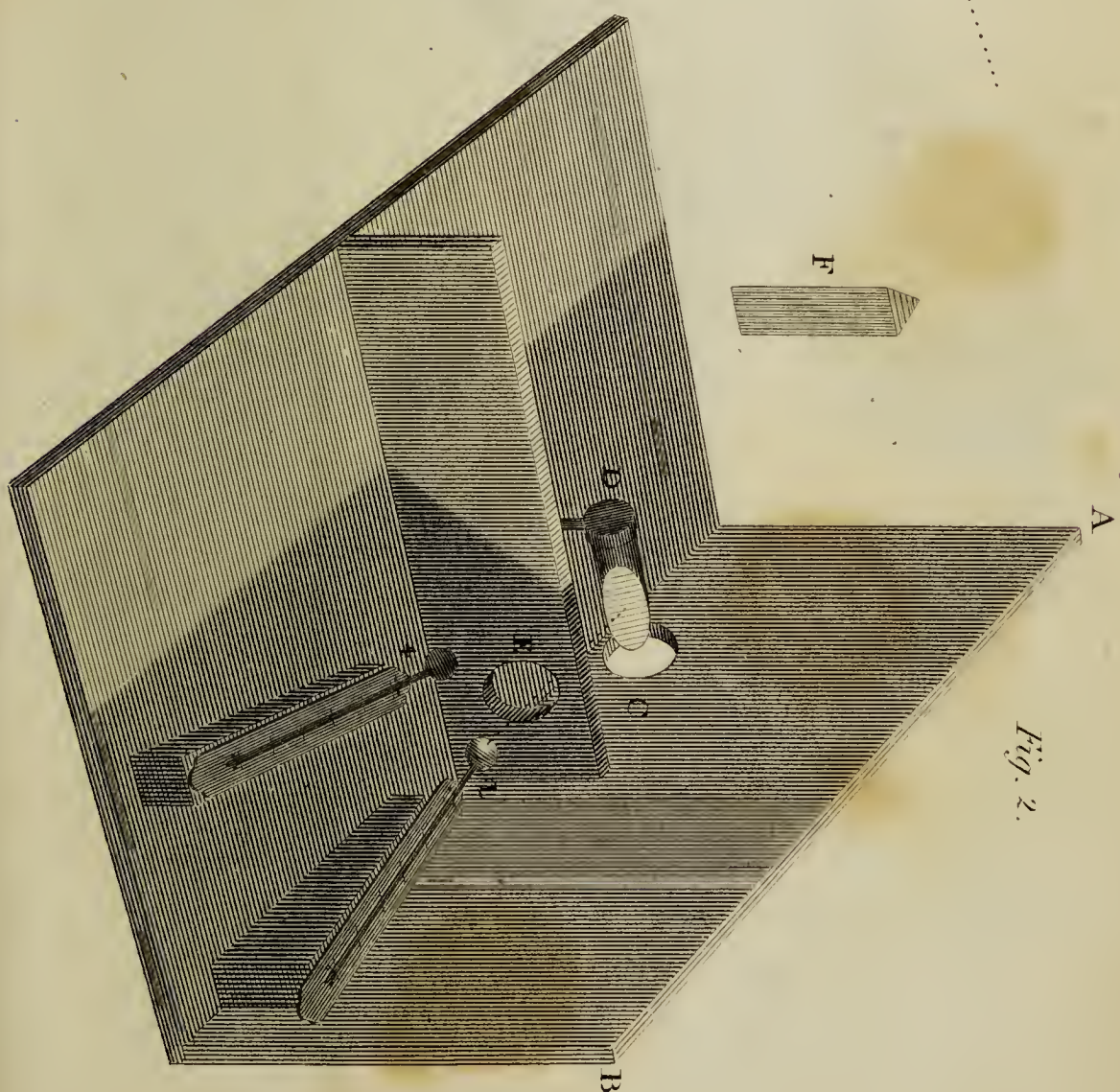




*Fig. 1.*



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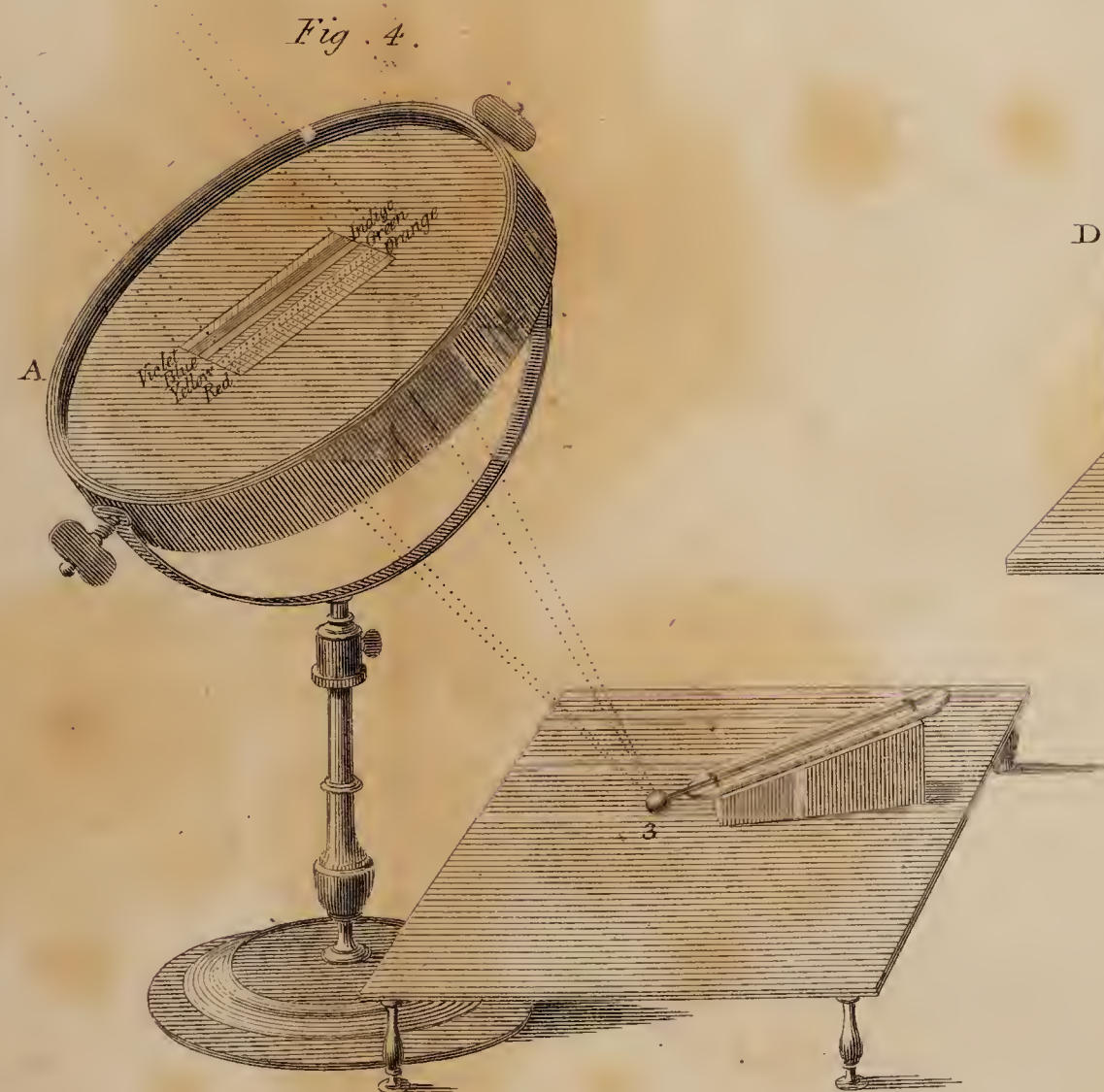
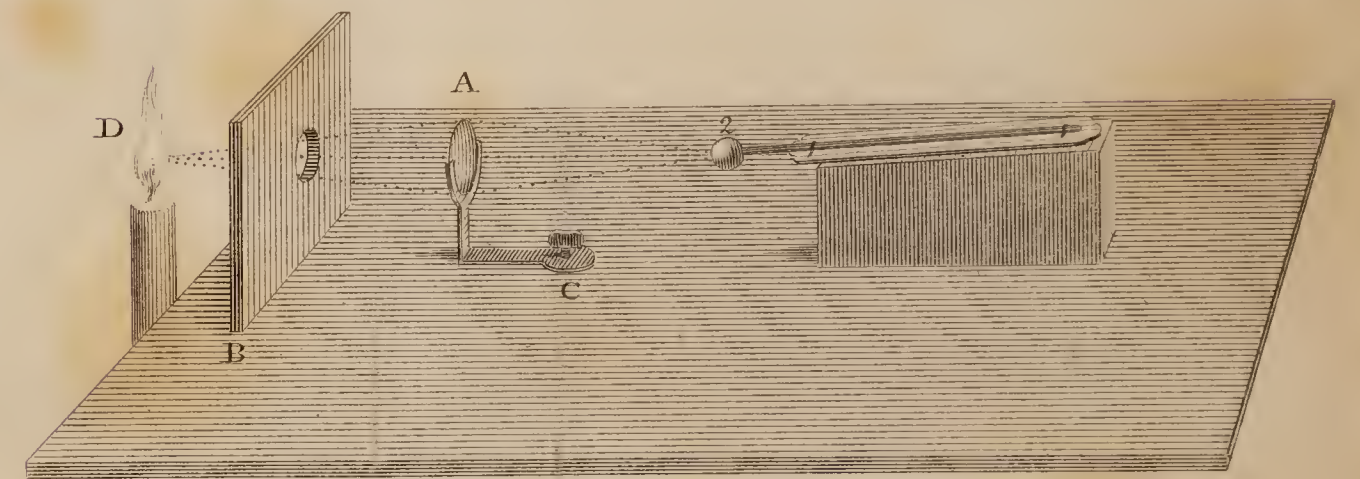
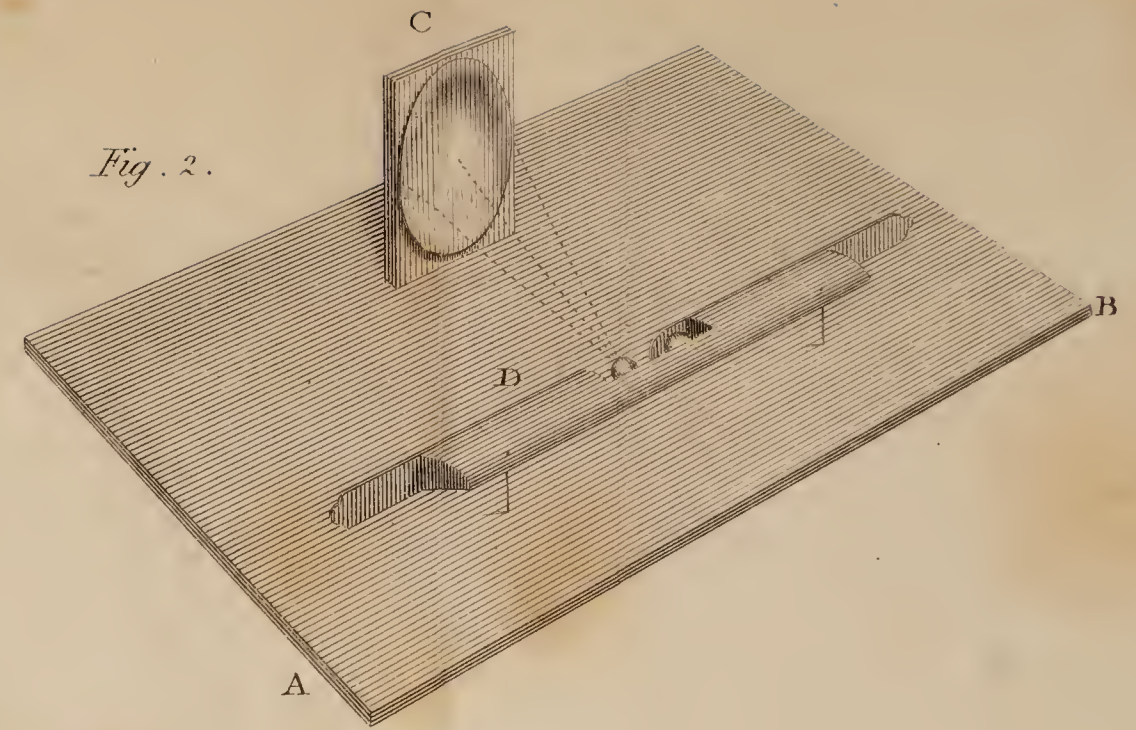
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*Lowry sculp*





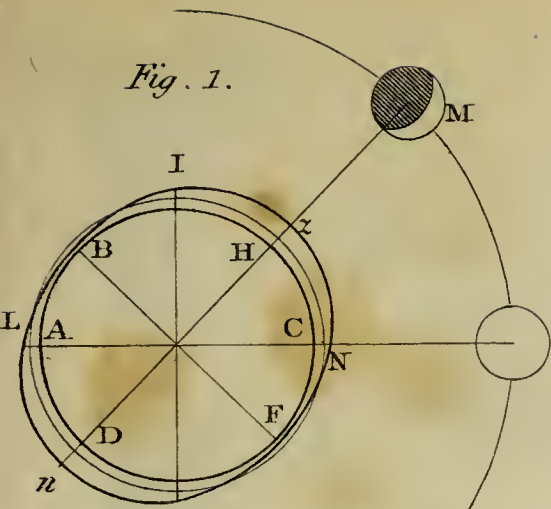




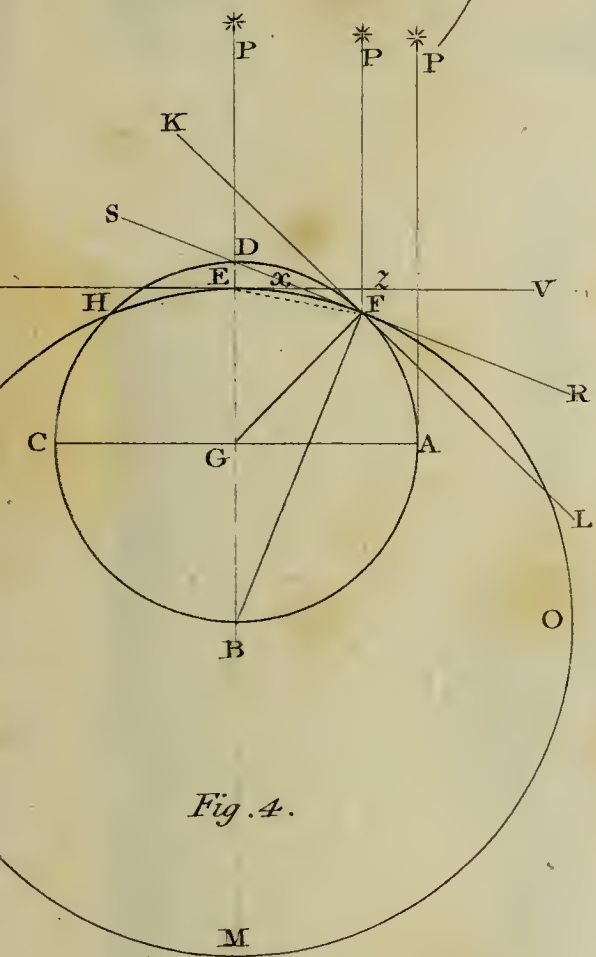




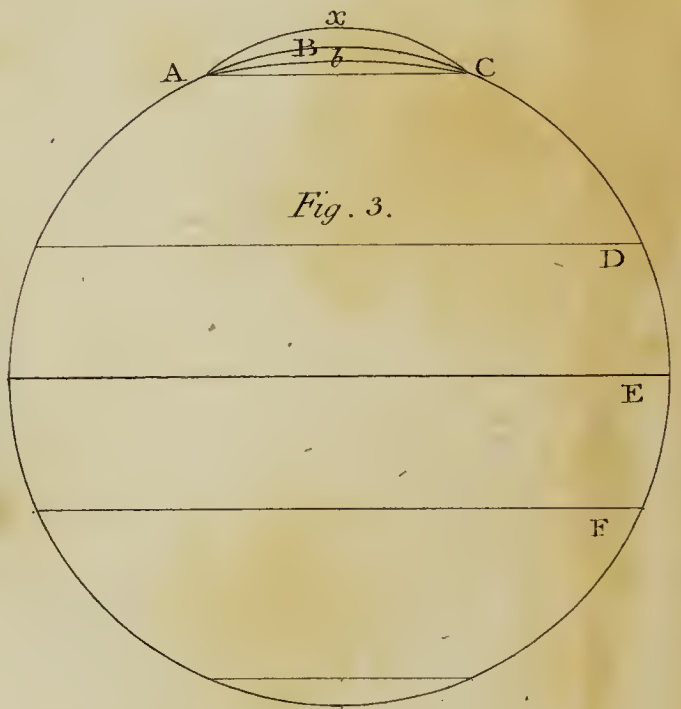
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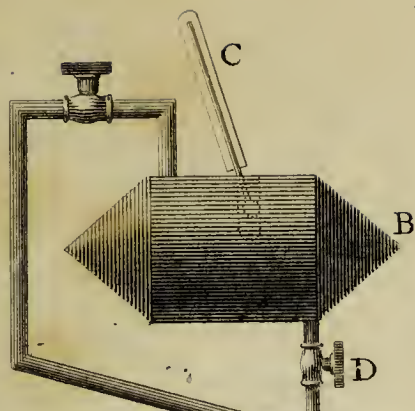


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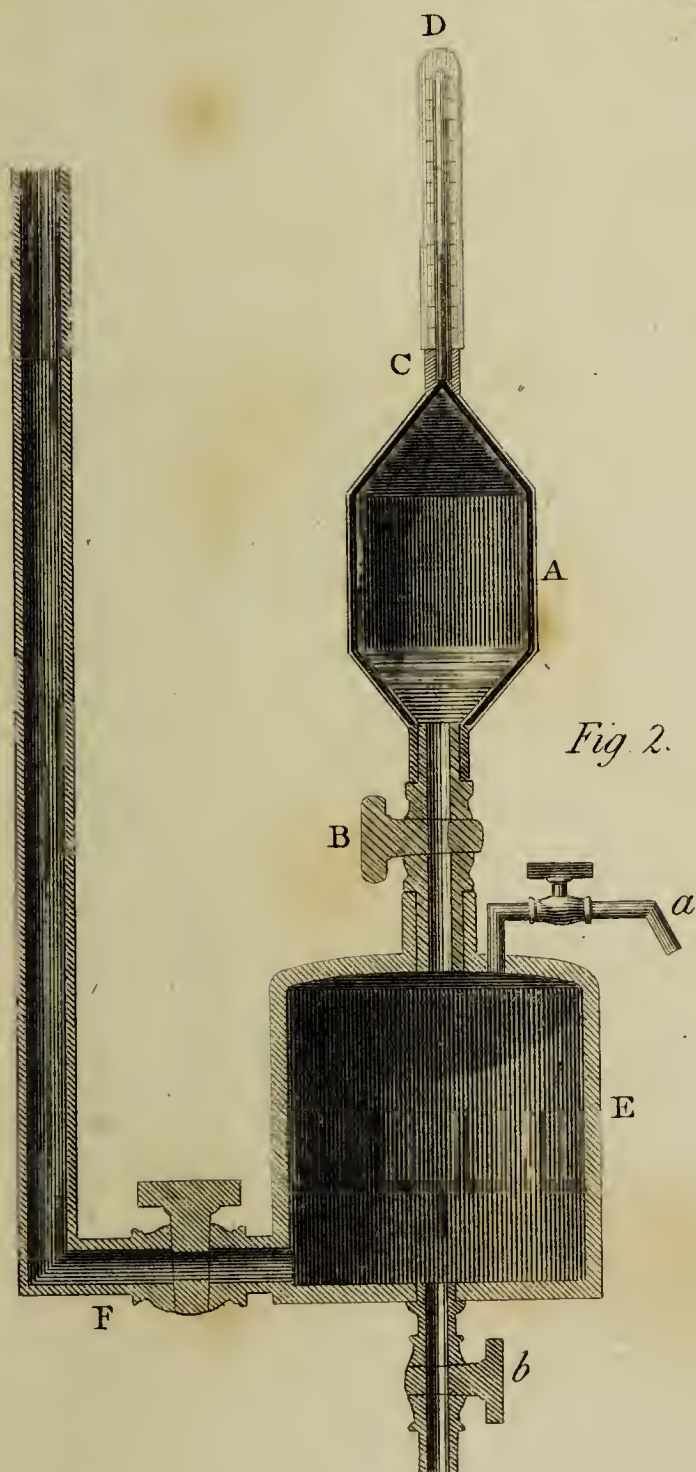








*Fig. 1.*

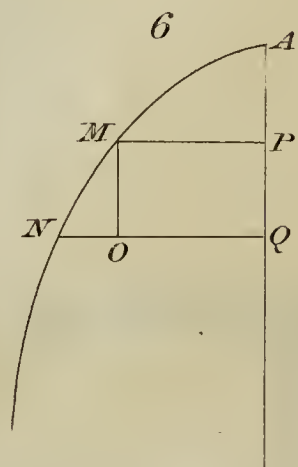
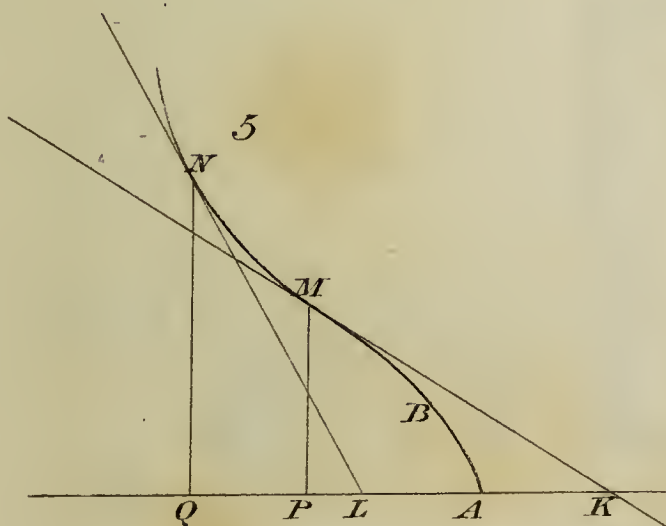
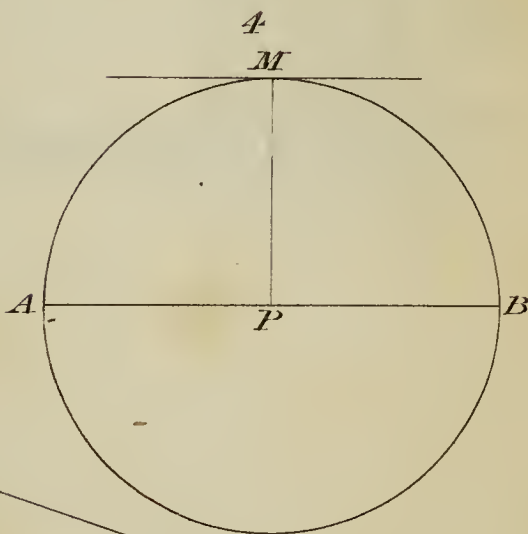
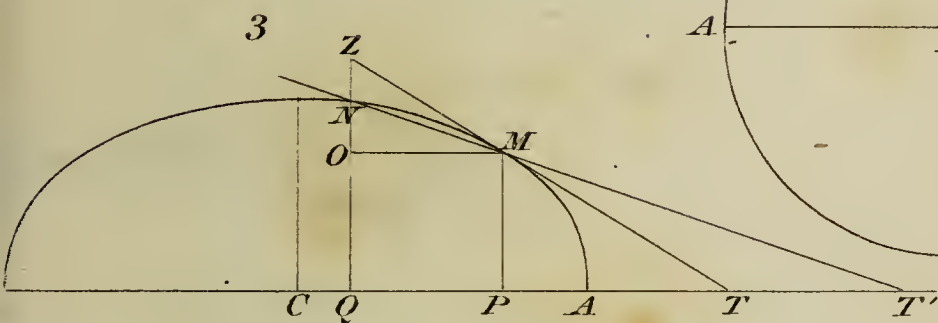
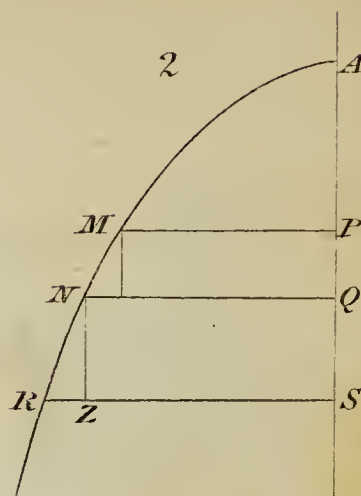
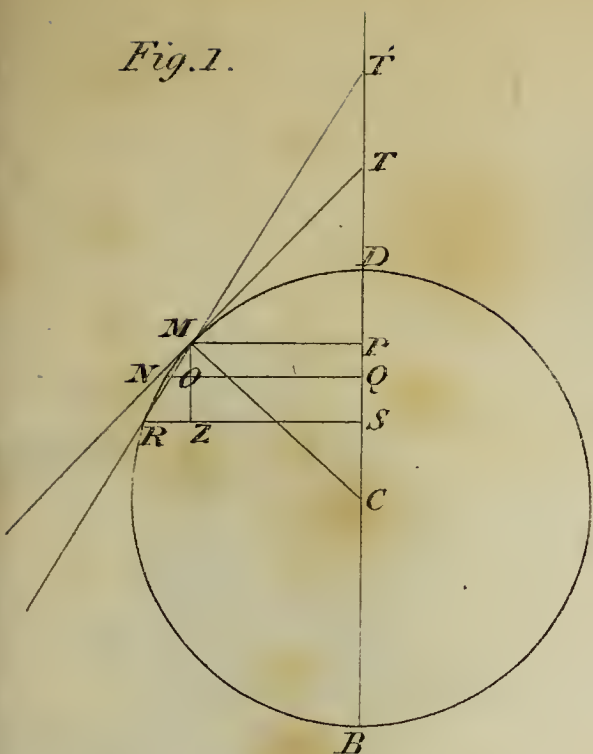


*Fig. 2.*





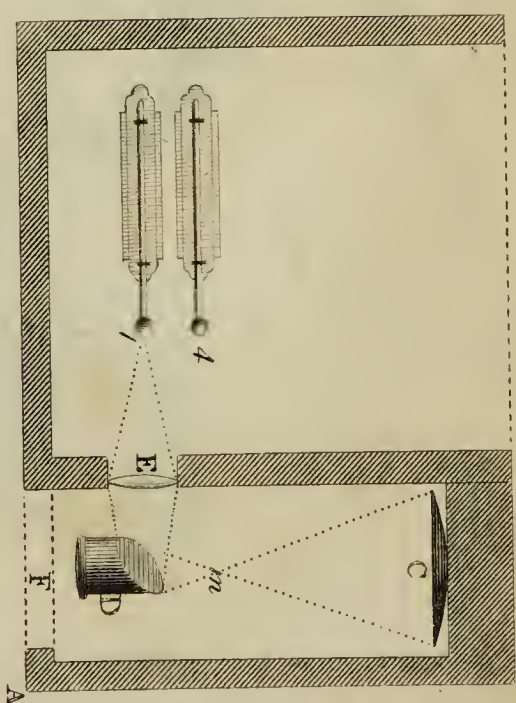
*Fig. 1.*



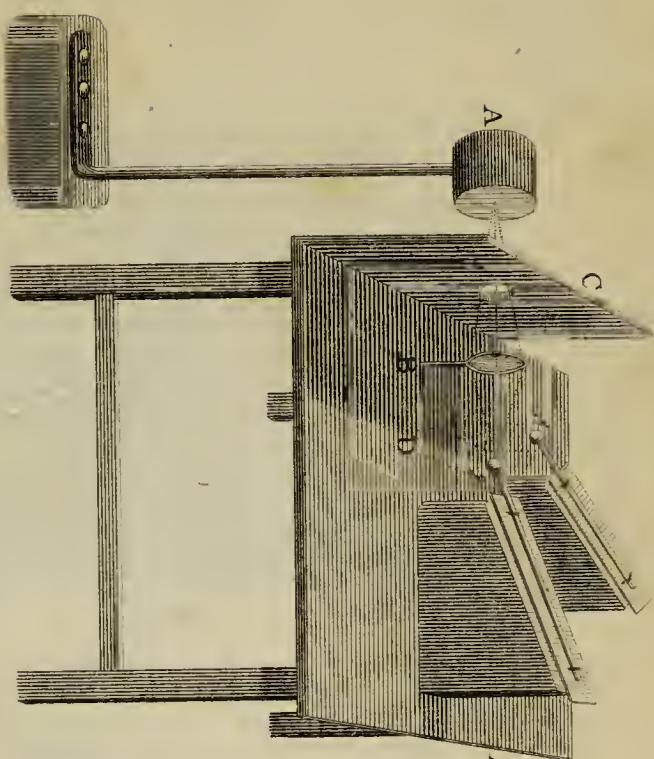




*Fig. 2.*



*Fig. 1.*



*Fig. 3.*

